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CONTENTS

- Mead and Freud: Their Relevance for Social Psychology
Guy E. Swanson 319
- Social Class and Childhood Personality.....*William H. Sewell* 340
- The Choice of Variables in the Study of Socialization
Eleanor E. Maccoby 357
- Sex Differences in Identification Development.....*David B. Lynn* 372
- The Diffusion of an Innovation Among Physicians in a Large City
Charles Winick 384
- The Social Dimensions of a Twelve-Man Jury Table
Fred L. Strodbeck and L. Harmon Hook 397
- The Effects of Continued Practice on the Behaviors of Problem-Solving Groups.. *Arthur M. Cohen, Warren G. Bennis, and George H. Wolkon* 416
- Toward a Methodological Codification: The Shotgun and the Saltshaker
Edgar F. Borgatta 432

SOCIOMETRY

EDITORIAL POLICY

Sociometry is concerned with the entire range of interests and problems represented by research in social psychology. It is the policy of the editors to seek those manuscripts for publication which represent the significant research interests of investigators who are concerned with giving the field of social psychology theoretical structure and reporting research which is clearly focused, well designed, and competently conducted.

While social psychology is presently regarded by most as a field with indeterminate boundaries, it has as its central focus the investigation of the processes and products of social interaction at the interpersonal, intrapersonal, intergroup, and intragroup levels and the development of significant generalizations therefrom. In keeping with the more general meaning of the name of the journal emphasis will be placed on measurement of social behavior. However, this emphasis does not exclude the acceptability of good articles which must rely upon qualitative materials and analyses.

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Mead and Freud: Their Relevance for Social Psychology¹

GUY E. SWANSON, *The University of Michigan*

Freud would enjoy and, doubtless, interpret the ambivalence of social psychologists toward George Mead and himself. Their ideas are viewed by many scientists with great deference and grave doubt. Mead and Freud are held to be both indispensable and incompatible. Mead's work is generally considered fundamental but without fruitful implications for research; Freud's as provocative but wrong.

It should be understood that all social psychologists are not deeply ambivalent about Mead and Freud. It is a symptom primarily of those who look to sociology as their intellectual home. The average textbook by a sociological social psychologist cites Mead and Freud more often than any of the other theorists who wrote extensively before 1935. Moreover, these books employ symbolic interactionism and psychoanalytic thinking as their major frameworks.

I want to examine the present and prospective roles of Mead and Freud in the development of a sociological social psychology. My intent is not that of offering an intellectual history. My objective is to clarify some of the potential contributions of Mead and Freud to a sociological social psychology, to locate the essential nature of the incompatibility and convergence between them, and to assess their prospective relations to empirical research. To accomplish this purpose in a limited amount of space, I deal with only some of the topics they treat. Because it seems the more fundamental, my focus is on their treatments of mind as a process rather than on their accounts of the individual's organization as a self or as a personality.

I should say at once that there are certain points which I intend to ignore. I accept as fact, but will pass over, Freud's tendencies toward instinctivism and Lamarckianism, his penchant for fictive histories of doubtful worth, and his lack of knowledge about, or understanding of, the differences between cultures. At the same time, I shall assume that it is consistent with the best evidence to say that Freud was usually correct in believing that his major conceptual distinctions represented important empirical discriminations.² I have no desire to join in that variety of revisionism which removes most of Freud's empirical observations while amputating instincts and the inheritance of acquired characteristics. I shall, in short, treat Freud, not neo-Freudianism.

Further, I shall not concern myself with the important controversies about the metaphysics and social values of pragmatism, functionalism, and psycho-

¹ Revision of a paper read at the 1960 meetings of the American Sociological Association.

² The evidence is surveyed in Blum (5).

analysis or with their relation to public affairs.³ My present interest is in empirical fruitfulness and technical adequacy of these theories for the problems of social psychology. What are those problems?

THE PROBLEMS OF SOCIAL PSYCHOLOGY

Social psychology is the study of the relations between human individuals and social organizations. The nature of these relations depends on one's conception of the parties involved. Because social psychologists have no consensus on this matter, the following statements represent only my personal preferences.

By "individuals" I mean, not organisms as such, but organisms engaged in selecting from, adapting to, and utilizing their surroundings. These processes are what Dewey and Mead called "mind." The organism's current potential for engaging in such a process is called its "personality."

The organism, as such, is significant for the individual on two accounts. First, its internal processes provide requirements which force it to import resources from the environment and determine the classes of environmental objects which may be so imported. Second, the organism is a self-stabilizing organization or system. This means that its internal processes operate in a manner that admits or rejects elements from the environment according to their likely effects on the system's stability. In this way, each organism's self-stabilizing processes afford a kind of unitary standard against which the environment is evaluated. Thus the sustained internal coherence of the organism provides one ground for the organization and coherence of the individual.

Social relations are the influences that individuals exert on one another. It is customary to say that one individual influences another only because he affects his associate's access to resources. Individuals have relations because they are resources for one another or because they affect the means for obtaining such resources.

Social relations are always orderly and, in that sense, organized. This does not mean, however, that they always are organizations. The term "social organization" refers to a social relation in which the individuals concerned so behave as to prevent the disruption of their mutual influences by extraneous events. (When this self-stabilizing process occurs with some awareness by the individuals of its existence and function, we speak of their relationship as a "group.")

It is usually said that organisms, individuals, and social organizations are

³ For a consideration of Mead's relation to these controversies, see Blau (4), Pfuetze (33), and Natanson (25). Important evaluations of Freud in relation to these controversies are found in Bartlett (3), Marcuse (21), Kaplan (19), and Rieff (34).

analytically separate from one another. This analytical separation is important primarily because it represents empirical independence. I shall take it as established that organisms, individuals and social organizations are independent in three senses: They are not reducible to one another, they exercise measureable constraints on another, and, in a given population over any given period of time, the variance in the internal processes of each of these three systems or organizations may show considerable independence from variations in the other two.

This empirical independence is not equivalent to a lack of relationship. It is a potential and, occasionally, actual independence which documents the existence of three different and equally real organizations. Thus we find that individuals provide resources required by organic systems; social organizations provide resources required by individuals. But, once each of these systems is in being, we can turn the matter around. As an organization or system, each has internal processes which operate to produce stabilization. Thus the requirements of groups for stability impose obligations and limitations on individuals who need to maintain those groups for their own purposes. In their turn, individuals, as systems, impose comparable demands and constraints on the organisms which they serve.⁴

Like the relations between individuals and social organizations, social psychology moves in two primary directions. It seeks, on the one hand, a description of individuals and of their relations to each other's resources that will explain the rise and properties of social organizations. It seeks, conversely, to define the requirements imposed on individuals by participation in social organizations and the consequences for individuals of their imposition.

Among psychological social psychologists, the greater portion have studied individuals and their relations as the source of organizations.⁵ A majority of the social psychologists trained in sociology show special interest in the effects of organizations on participating individuals.

Social psychologists have sought to understand the rise of organization among individuals through the study of interpersonal influence, power, communication, symbols, and the media of communication. Their work on elementary collective behavior, as well as many laboratory investigations of small groups, illustrates the same concern.

A different set of investigations illustrates efforts to explain the effects of organizations on individuals and to state the conditions under which these occur. Organizations persist if individuals depend on their presence, become responsible adherents to their norms, and contribute to the solution of their

⁴ Some of the evidence especially relevant to this point is reviewed by Olds (26).

⁵ This variety of approach is illustrated in Floyd Allport's early work (1). Perhaps the most recent survey and synthesis appears in Thibaut and Kelley's book on groups (39).

problems. The social psychologist's concern with socialization, self-control, and commitment to the group reflects his interest in the effects of organizations on individuals. So also does his effort to explain how values, attitudes, and opinions are acquired and gain some degree of order and stability as indicated by the rise of individualities, identities, and selves. The explanation of individual opposition or indifference to organizational requirements has been of special interest, as has over-conformity to those requirements.

The properties of social organizations play a special role in organizing sociological work in social psychology. It is those properties which distinguish social organizations from everything else in the environment that the social psychologist pursues as conditions that shape the mind. It is those features of mind relevant for explaining these same distinctive organizational properties and their variations that are of concern as he seeks to account for the rise of groups. In short, the defining properties of social organizations, and variations on those properties and on emergents from them, afford the coherence in a sociological social psychology. Among the candidates for inclusion in a list of such properties are social norms,⁶ systems of symbols, sentiments conceived after the manner of Cooley, and Bales's "system problems."

THE PROBLEMS OF MEAD AND FREUD

Neither Freud nor Mead set out to develop a social psychology. The philosophical tradition which Mead sought to advance defined its task as that of clarifying the relation between knowing and being—between epistemology and ontology—between the sentient organism and its environment (24). Interest was focused on the conditions of a valid knowledge of being. Valid knowledge was conceived as rational, intelligible, and self-conscious. This conception led, in turn, to a search for the empirical connections among signs, thought, organic action, and the environment, and to a concern with the universality of ideas and the reality of relations. Mead's social behaviorism finds its major problem in accounting for rationality, or, as he liked to call it, "reflective thought." His special contribution is an explanation for the varying access which men may have to the covert parts of their own acts and the consequences of such access for the effectiveness of man's relation to the environment. Mead locates the foundations of rationality in symbolic responses which people provide for the covert aspects of each other's behavior. If those responses are absent, men cannot think reflectively.

⁶ In the sense employed by Durkheim. His definition and its implications are reviewed with unusual clarity by Peristany (32).

Like Mead, Freud wanted to account for rationality. Whereas Mead was concerned with the irrationality which appears because the environment does not respond properly to the individual's inner life, Freud sought to understand the irrationality produced by subjective conditions which prevent the individual from properly interpreting the world around him. The personalities of his patients prevented their experiencing gratification.

Because Mead finds the locus of irrationality in the absence of certain environmental events, his major focus is on the peripheral processes of behavior—on perception, the selection of stimuli, the character of the reinforcement provided by stimuli, the reformulation of instrumental acts, and the release of blocked impulse into the environment. Freud, on the other hand, wants to change the balance of forces internal to the individual with the aim of removing subjective conditions which block a correct appraisal of the environment. This leads to a conceptualization of the principal components of personality and of the distortions which their relations impose upon the individual's contacts with the environment.⁷

It is evident that Mead and Freud had quite different aspects of behavior at the center of their attention. The wide difference in their interest is a major reason why each of them retains a special claim on our attention.

MEAD, FREUD, AND SOCIAL PSYCHOLOGY

We have seen that the development of a social psychology involves certain steps. Organism, mind, and social organization must be differentiated from one another and their relations identified. Minded organisms—individuals—must be so described that their relevance for organizations, whether as creators or as objects, is readily exposed.

Neither Mead nor Freud has much to offer us in the way of a description of social organization. Neither presents any detailed technical treatment of the rise of organization out of interaction. Both do something to distinguish and characterize organisms and minds. Both say a great deal about the impact of organizations on individuals. What of value does each have to offer us and how does each compare with the other? Let us begin with Mead.

Mead

From the beginning of their acquaintance with Mead's work, many sociological social psychologists valued two of its features. The first was its sophisticated presentation of a functionalist psychology. The second was its bold

⁷ There is some justice in the charge that Freud was mechanistic in his treatment of impulses, attitudes, and the like. On the other hand, this treatment does much to capture the perseverating and inflexible character of his patients' behavior. For discussions of Freud's mechanistic tendencies, see Allport (2), Bartlett (3), and Osborn (27).

extension of functionalism to explain certain of the individual's inner processes, certain of the covert aspects of acts.

a. *Functionalism*

Since its inception, functionalism⁸ has been the perennial American psychology. Many of its principles became so well accepted as to be unquestioned. Heidebreder (15) said in 1933:

... functionalism does not, at present, stand out in American psychology as a distinct school and system. It did so only in its beginnings, when it had the conspicuousness of a new movement—in particular, of a movement opposed to structuralism. . . .

... in functionalism, American psychology passed through a phase of its development in which it brought together and organized many tendencies already in existence, utilizing them so successfully that they passed into general practice. To treat of mental activities as well as contents, to think in terms of adaptations and adjustments, to observe psychological processes in relation to their setting, to regard man as a biological organism adapting itself to its environment—all these procedures have been so widely accepted in psychology that they no longer attract special attention.

If anything, functionalist views are even more firmly rooted in academic psychology today. Perhaps the most dramatic evidence for this is the steady revision in functionalist directions of Hullian learning theory, the closest lineal descendent of Watson's behaviorism (16, 23).

Functionalism was a psychology well adapted to the requirements of sociologists and social psychologists. It fitted both their data and their theoretical tastes. More than this, it had survived the rigorous test of explaining the same phenomena treated by competing schemes while avoiding their errors.

Functionalism was not rooted in elementarist or mechanical premises. The relations among men or attitudes or particles were just as valid in ontological status as the men, attitudes, or particles themselves. Moreover, the individual element, seen from the standpoint of the organization or system in which it participated, had meaning as a part of that system—had a role in the system. This role was a condition of being, equal in validity to the element's role as one of the ingredients from which the system first arose. Here, then, was a sophisticated philosophy that agreed with the sociologist's social realism.

By granting ontological independence to relations among elements and by seeing the nature of elements as transformed by their involvement in such relations, functionalism made tenable the position that general ideas could possess empirical independence and validity. This conclusion supported those who wanted to take seriously, and treat as independently variable, the special class of general ideas designated by terms like symbols, social norms, and culture. Faris (10) and others quickly understood that here was an out-

⁸ Functionalism's origins are traced in Blau (4), Hook (17), and White (40).

look which credited with full reality and potency "all that is noble in us as well as the ignoble" and enabled one to treat "the emergence within the actions of men of what we know as distinctly human."

The functionalists also provided conceptions of behavior itself that were congenial to sociology. Of particular importance was their description of the relation between body and mind.

Sociologists had already been impressed with the variability of behavior between cultures and between different periods in the development of given cultures. Any psychology congruent with these observations must allow for considerable malleability in behavior. Functionalism did just that.

It pictured the human organism coming out upon the world's stage equipped with a certain range of sensory capacity and with that vague impulsive quality which signalizes life. The living organism, by virtue of being alive, must have some intercourse with the environment. It is not self-sufficient and must engage in a constant interchange of energy with the environment. The organism moves ceaselessly, but only those features of the environment which it is equipped to perceive and which, additionally, impede its passage are of interest for the psychologist. Only toward these does mind arise. The psychologist's task, as Mead and other functionalists saw it, was to assume life and to explain how it comes to take one direction rather than another. Direction is a movement toward or away from impediments. Life so directed is mind.

This vision of man pictures him as originally innocent of all knowledge concerning his world. It describes conditions that require sustained relations with that world, and provides the environment with a significant role in determining what those relations will be. But it does more.⁹

It describes some of those relations as instrumental. We must remember, in this connection, that the functionalists were providing an account of mind, not of behavior. An instrumental relation—mind—connects the organism and

⁹ It would probably be correct to say that the topics of special interest to sociologists in the 1920's and 1930's enhanced their enthusiasm for Mead. Their concern with urbanism as a way of life and with social marginality turned attention to the conditions under which man could be a problem to himself, to discussions of identity and individuality. Mead provided terms and principles for this discussion. The same interests in urbanism stressed the difference between ecological and normative relations—between interaction not mediated by common rules and symbols and interaction in which these media were present. Mead stressed this same distinction and provided an explanation of its consequences. Again, the twenties and thirties were years in which, like other American intellectuals, sociologists sought to find a place in scientific explanations for morality and religion, truth and honor, ideas and affect, history and aesthetics. This place had to be one which did not reduce these entities to epiphenomena. Mead, like Dewey, James, Peirce, and Cooley, sought to provide such a place.

the environment. If we consider the individual seeking to establish a viable relation of this kind, he is, by definition, rational. By this I mean that he is, by definition, seeking means fitted for his ends. Means which are not fitting, in this sense, will be rejected. Mind, so conceived, is indeed a malleable affair.

This malleability is further enhanced by another feature of the functionalist's conception. Mind, as they liked to say, lies between organism and environment. Many features of the environment change regardless of the organism's stability. Therefore, whether the individual recognizes it or not, mind also changes.

It is worth remembering that this malleability granted to mind follows inexorably from the way it is defined. The functionalists were saying: If individuals seek those relations to the environment which permit the release of a given impulse, the relations chosen will be those known and available alternatives that suffice. The organism is presumed to be ready and willing. The environment determines what ends can be realized and under what conditions. The functionalists simply did not treat the problem of the intra-individual conditions that determined whether and when the individual would take the first steps to establish instrumental relations.

From this same definition of mind there follows the characteristic functionalist attitude toward the conceptualization of "reinforcement." The typical learning psychologist conceives the environment's reinforcements as rewards or deprivations, and describes them in such terms as recency, frequency, regularity, and amount. The classical functionalist did not find this description wrong. It was incomplete. These bare categories are inadequate for describing the variable relations between impulses and environment of which mind consists. They do not enable one to differentiate between love and lust, between fear and hate, between an aesthetic judgment and an ethical decision. From a functionalist view, the concepts conventional in learning psychology inform us only of the degree of fit between some impulse and the environment, and the probability that this fit will recur. Such information is necessary, but insufficient for a description or explanation, however general, of mind and its variations. It is quite clear that two instrumental relations can be identical with respect to these learning variables and very different in all other respects.

An adequate conceptualization of the dimensions of mind is still lacking. We can understand the social psychologist's flirtations with Kenneth Burke's dramaturgical vocabulary or with Lewin's properties of the life space or with Parsons' categories of action as efforts to construct appropriate conceptions.

What all of these efforts have in common is the description of the distinguishing features of an instrumental relationship—what Dewey and Mead would have called the description of the "whole meaning." The sociological

social psychologist is likely to believe that humans learn about social relations, not by fitting together bits and pieces of experience, but by grasping the general character of roles and norms. If roles and norms are instances of mind, there is no need to seek in neural associations for the process by which they are constructed. Instead, the association among their elements is interpreted as instrumental, not neural, in character. Functionalism shows how this can be so.

I have said that sociologists found malleability and "whole meanings" in behavior and appreciated functionalism as a psychology which took them into account. Functionalism was suited for many other purposes as well.

Sociologists often conceived of social relations and organizations as instruments that enabled individuals to obtain what they would have lacked without sustained help from other people. Functionalism was the psychology of instrumental relations. Sociologists were impressed with the importance of the cultural inheritance in determining man's view of his environment. Functionalism, as an instrumental psychology, dictated that the environment played a part in conduct only if it was relevant for the organism's requirements, including those requirements it had gained through living with other people and learning from them.

It has sometimes been charged that functionalist social psychology advances two incompatible pictures of mind. On the one hand, it sees mind as easily changed, on the other as locked forever in its own internal functioning, prevented by its definitions from encountering the outer world.

What this criticism refers to as incompatible visions of mind actually represents earlier and later stages of action. The organism is conceived as attending only to those objects that are relevant for the release of impulse. However, once the organism constitutes some object in the environment as a stimulus, once, that is to say, it perceives some object in the framework of its impulses or motives and describes its instrumental relevance, the properties of that object play a part in conduct. They either validate or invalidate the organism's instrumental hypotheses. A line of action persists as long as it continues to be validated by the environment. It stops when the environment provides no reinforcement or negative reinforcement. Organisms that perceive the same environment in the same fashion will receive the same reinforcement from it.

Mead's account opposes the view that conduct is determined solely by the organism or by the environment. It is not opposed, in principle, to a deterministic view of behavior. Organic necessities and past experience determine the hypotheses which the organism advances in the early stages of action. Once stimuli are constituted, they write their responses to the organism's proposals in a firm, round, determining hand.

We find, then, that functionalism has multiple appeals for sociological social psychologists. It readily enables the treatment of social relations as instrumental. It affords a significant role both to a determining environment and a selecting individual. It provides a clear distinction between mind and body. It agrees with the sociological disposition to take relations as ontologically valid and to search for whole meanings in order to understand and predict.

b. Functionalism and Intra-Psychic Events

It is with functionalism's treatment of intra-psychic events—of the covert stages of action—that we come to Mead's special contributions to social psychology. Because functionalism views mind as a relation between organism and environment, all special aspects of mind are also such relationships. These include perceiving, attending, evaluating, and responding. These processes become intra-psychic events in the form of expectations about the world which are acquired from participating in it and learning about it.

When Mead took this general functionalist approach to mind and applied it to reflective thought, he was forced to locate instrumental relations between organism and environment that might provide the special instances of mind represented by self-conscious problem solving. The grounds on which he concluded that only human interaction mediated by the standardized symbols of language could provide such relations are too well known to require review. They indicate one significant method by which the *dynamics* of social relations, as well as their elements, become a part of mind. We shall return to this point in discussing Freud.

Freud

Like Dewey and Mead, Freud appreciated the importance of the mind-body problem (18). Like them, he evolved a position which provides a significant place both for human biology and for the environment in the determination of behavior. Freud, like the functionalists, pictures the adaptive behaviors of the organism flowing from motives, with stimuli being constituted, as such, by the perspective which motives afford.

There are further similarities. Both Freud and the functionalists conceive of each act as the product of a total situation, not of isolated stimuli or single impulses. Both believe that behaviors persist only because they contribute to the organism's survival. Both, though with somewhat different emphases, deny the validity of introspection as a tool for investigating conduct. Both distinguish sharply between the original nature provided by human biology and the human nature produced by man's experience with his fellows. Finally, Freud comes closer than is often appreciated to the functionalist position

concerning the indeterminate character of the newborn's psychic life (5). Mead and Dewey stress the formless nature of original impulsivity. Freud separates libido as a kind of unorganized and impressionable impulsivity from those tissue states like hunger and thirst in which a periodicity of appearance is determined more by biology than experience and for which the range of suitable environmental reinforcements is quite narrow. By contrast, the organism's libido or sexuality is a generalized propensity to act, gaining structure and differentiation by virtue of its history in the environment. As Sartre (35) observes, "The libido is nothing besides its concrete fixations, save for a permanent possibility of fixing anything whatsoever upon anything whatsoever."¹⁰

We must turn once more to the problems of central concern for Mead and Freud to understand the essential differences between them. Mead sought the conditions of valid knowledge and defined such knowledge as the appropriate alignment of means and ends, the alignment of the act in its covert and overt aspects with the objective environment.

As we have seen, Freud's concern was with gratification. Now gratification is not a property of mind. Instead, it refers to the net balance of the organism's profits and losses resulting from his transactions with the environment. Mind is a means from which this balance emerges. Mead takes it as objectively problematic that acts and objects can be well aligned, but assumes that gratification will follow if they are. Freud assumes that such alignment is possible, but finds alignment, as such, insufficient to assure gratification.

Mead tells us how impulses, once aroused, become related to objects with which they can effect an interchange of energy. But Freud must explain why some individuals seem unable to express impulses and why others cannot modify their conduct as a result of encounters with objects. Freud would agree that, once an impulsive process is involved in becoming related instrumentally to objects, the character of the objects does much to determine the nature of the relationship that emerges and the likelihood that this impulse will arise again or in its present form.¹¹ Mind, as defined by Mead would be recognized by Freud as changeable. But Freud was provoked by those conditions before and after mind which prevented it from arising; which produced, not instrumental relations, but other behavioral processes. Those processes, separated from experience, are not readily changed by it.

To pursue his objectives, Freud moved in several directions. He sought, first, to characterize impulsivity prior to its shaping by contacts with the

¹⁰ McDougall (22), who welcomed Freud to the ranks of hormic psychologists, was specially critical of this indeterminate character of many Freudian "instincts."

¹¹ See, for example, Freud's (13) treatment of ego, the reality principle, and secondary process. A convenient review appears in Fenichel (11).

environment. He tried, second, to explain why some parts of the body were found more often than others to be centrally involved in the organism's gratification. He worked, third, to characterize the environment in terms of its potentialities for limiting the full expression of impulse. Fourth, he described certain common relations among impulse, erogenous zones, and environmental limitations. (Seen as stable patterns in a state of equilibrium, these relations are the Freudian types of character—orality, anality and the others. Considered as a self-stabilizing arrangement restoring equilibrium after its disruption by impulse or environment, these relationships are the mechanisms of defense—projection, denial, reaction formation, and the rest.) Finally, Freud developed a theory concerning the conditions sufficient to permit the individual to experience the available gratifications—a theory of therapy.

What were Freud's conclusions from these explorations of the psychology of gratification? What is their relevance for a social psychology?

In Freud's description of impulse, we find a first condition that prevents perfect gratification. Original impulsivity knows no limits. Its source is in the internal requirements of the organism. The environment cannot provide for the full satisfaction of those limitless requirements or offer adequate compensatory satisfactions for those it denies. Given the nature of impulse, not just civilization, but any environment, is a source of discontents.

Other things being equal, such discontents are likely to become associated with the orifices of the body because these zones provide individuals with maximal pleasure. Instrumental relations with the environment have these zones as their primary foci because pleasure and pain are greatest there.

Although of lesser importance as sources of pleasure, other parts of the body have some potentialities of the same order. The eyes, ears, fingers, and limbs are relatively important. Presumably the ear lobes and the small of the back, lacking as they are in receptive sensitivity and instrumental relevance, are among the least significant sources of pleasure.

The body's zones differ not only in the degree of their erogenous potential, but in the *mode* by which each affords gratifications from contact with objects. These modes are instrumental relations shaped by the physiological properties of the several zones. Thus gratification by way of the mouth and lips involves sucking, chewing, licking, tasting, and incorporation. Gratification by way of the genitals involves insertion or reception.

Freud understood perfectly well that the organism, as a totality including its acquired skills, and not just these special zones, was the source of many gratifications. For the weight-lifter, the muscular development of the small of the back may be greatly prized; for the Kulya woman, her earlobes, grossly distended by bronze rings, are of great significance. For the intellec-

tual, the quick mind and the apt phrase are matters of pride. But, Freud insisted, these sources of pleasure are added to those that biology originally provides, and the acquired sources become available much later in life. The socialization of the erogenous zones, coming first, establishes, as it were, a set of premises with which later socialization must contend. First, unless the individual is adequately gratified from these original sources, it is unlikely that he will have the energy available to venture a search for gratification from less immediate and less concrete sources. Second, if the mode of gratification from a later and more abstract source has features common to the mode built into some erogenous zone, some association is likely to arise between them. This association is illustrated in the famous formula (14) by which a child is proposed as the equivalent of a penis, which is shown to be equivalent to feces. It is not a matter of the individual confusing these objects with one another in perceptual or cognitive terms. Were that to occur, it would indicate grave pathology. What Freud does say is that these three objects come to be equivalent with respect to the mode through which they give pleasure to anal women.

Freud was more convinced than most of us are likely to be of the potency of the original erogenous modes for determining the nature of all later gratification. It is difficult, however, to dismiss the existence of many associations between these earlier modes and those which come later. Common figures of speech, verified interpretations of projective symbols, and clinical records provide countless illustrations of such associations (12).

In any case, one must add such associations to the inherent insatiability of impulse as sources which frustrate gratification. According to the best evidence available to Freud or to us, such associations would emerge from the phasing of neural activity. Unlike associations that arise in instrumental relations between impulse and objects, these associations between attitudes are not an aspect of mind. They are not governed by instrumental laws and utility functions, and their occurrence interferes with the operation of such laws and functions.

Consider, next, Freud's description of the environment. It is given in terms of that environment's limitations to pleasure. If those limitations make it dangerous for the individual to indicate that he has a given impulse, whether he expresses it or not, they have the quality of superego. If the limitations merely qualify the timing and means of impulse expression, they have the quality of ego. Now we are back in the realm of mind—of instrumental relations, but these relations are classified from a perspective not considered by Mead, and, what is more important, Freud's categories raise novel problems for mind's future expression.

Like Mead, Freud observed that only other people respond to our impulses

as distinguished from the overt phases of our acts. Consequently, only other people will provide limitations with superego qualities.

A distinctive consequence of superego limitations is that, once they are accepted, the process we call mind vanishes, but the impulse need not. It is not merely unimplemented, it is defined in principle as unimplementable. The further associations it develops with other impulses and attitudes and the transformations it undergoes seem to owe little to instrumental associations and much to the internal, self-stabilizing processes of the organism.

Looking back over this sketch of impulse, body zones, and modes, and superego, we see that each involves considerations that fall outside the scope of a concept of mind as presented in functionalism. Yet none requires the assumption of instincts—of instrumental action established solely through biological inheritance. In a variety of ways, each of these three impediments to gratification calls our attention to non-instrumental, intra-organic events that precede, follow, or accompany mind, relating to it but having a significant degree of independent variance. We are reminded once more that Mead and Freud supplement each other's work by examining somewhat different phases of behavior. Because Freud wrote on such matters, we can be certain that he would have agreed with the functionalists concerning many of the properties of mind.¹² He would, however, have seen those properties as insufficient to account for psychopathology.

The varieties of character and the mechanisms of defense are the final result of Freud's work. Here all the ingredients previously sketched become organized in a systematic fashion. Freud, as Frenkel-Brunswick said (12):

... tends to view character structure from a defensive point of view, and social influences as a series of traumata which bring to a halt or discontinue instinctual gratification and expression.

... This view does not do justice to all the satisfactions gained from moving along constructive social avenues.

A number of somewhat independent observers have come to a similar view of the types of character. Erikson (9), Fromm (15), and Parsons (29, 30), for example, propose that these types represent residues of the major stages of socialization.¹³ Only Parsons offers a coherent account of the appearance of these particular types of character and of the order in which they arise. In essence, he suggests that each type of character represents the skills required to perform adequately in one of the four major roles embodied in social systems. These roles—these organizational facts—provide the key theoretical apparatus for explaining the rise of these types of character. We

¹² Freud's discussion appears in (13).

¹³ The interpretation that follows was developed originally in Swanson (38).

may note, first, that we are now confronted by a truly social psychological account and, second, that this account pictures these particular types of character not merely as relevant for participating in some organizations but as descriptions of skills essential for participation in all organizations. One version of this approach may suffice to indicate its general character.¹⁴

In interactional terms, dependency is the central fact of orality as a stage of development or as a type of character. From the standpoint of its parents, the infant is functionally dependent upon adults for its survival, but is unaware of its need for social support. It gradually learns that it is dependent. If the infant does not learn to be dependent, there is no foundation for the remaining stages and they will not appear.

The central interactional fact of anality is responsibility. Once the young child is committed to depend on others, they can and do make demands on him which facilitate their living together. The child must accept some responsibility for his own conduct, must exercise self-surveillance, or stable, continued interdependence is an intolerable burden to his elders. There is general agreement that most children find these responsibilities difficult to accept, and come only gradually to feel comfortable in living with them.

Observers are also in agreement in believing that the requirement for a child's assumption of stable participation with peers and others outside his family forces the generalization of dependent and responsible relations to these additional groups. Parsons has given us an especially sensitive and detailed picture of the way in which the structural demands of the family as an organization, the ultimate requirements of the total society for more mature forms of social participation, and the child's current interpersonal skills all interact to produce first the oedipal crisis and, if that is resolved successfully, the golden years of latency which take us from the fourth or fifth year to the brink of adolescence. It is plausible to regard such a sequence as common in all cultures with, perhaps some variations in timing. It is important to understand that the basic anal character, dependent and responsible, is retained throughout these stages. It is simply extended in the social relations to which it applies.

It is toward the end of childhood that another ingredient is added to this anal pattern, the ingredient of independence, or, more suggestively, of competent performance. This is the particular sign of the phallic character. We may believe that relations with peers and others outside the family are of special importance in this development.

By independence, I mean that the child is expected to be able to show sufficient self-confidence to enter easily upon new tasks and new social rela-

¹⁴ For another characterization of the interactional significance of these stages, see Swanson (36).

tions without looking to his family for constant guidance and nurturance. This is not primarily a matter of possessing appropriate intellectual and motor skills, although these are necessary. The focus of the phallic period is the demonstration that one can establish new social relations as the occasion demands and participate responsibly in them. These abilities are the fruit of latency and the foundation for the final step—the emergence of the genital character.

The genital character is dependent, responsible, and competent. In addition, he makes positive contributions to the development and maintenance of the groups to which he belongs. The description and explanation of genitality are the least developed in existing discussions of Freud's typology. Fromm catches the spirit of the matter in speaking of genitality as "the productive orientation (15)." I take this to mean that the individual feels obligated not merely to obey some limited set of rules, but to enhance and add to the lives of his fellows. He is no longer only a beneficiary of society, but one of its fully responsible operators, his welfare bound inextricably with the welfare of the whole organization.

I have been describing the normative and desirable aspects of these four types of character. Each has, of course, its deviant forms. But, in desired form or otherwise, the great sweep of Freud's typology is from dependence to contribution, from rudimentary commitment to social relations with one's parents to commitment to ever-larger groups, from conformity to set demands to an obligation for the performance of whatever services may be necessary for social growth and survival. From the individual's perspective, these stages look somewhat different. Freud was always alert to the competition between individual needs and social demands. As sociological social psychologists, however, we may view the sequence from the organizational standpoint, seeing in it the growth of the individual's social commitments.

This social, and, we may note, functionalist, account of Freud's characterology helps us to identify its powers and limitations. It touches on only a small part of the phenomena of development and socialization. That part, however, is of central importance for sociology. Whatever else we may say of them, social roles represent positions in a pattern of influence among actors, and influence, in its turn, rests on dependence. More than this, moral, stable, and continued influence requires responsibility, competence, and contribution. Freud's typology enables us to describe significantly different readinesses with which individuals characteristically engage in such relations (36). When, as often happens, we find individuals who seek roles that are unusual in the general character of the influence which their holders exercise or receive, we can have some assurance that Freud's types will allow us to characterize those individuals in a fashion that complements our description of their roles.

It was Lasswell (20) who seems first to have recognized and employed this special affinity of the psychosexual character levels for describing personality in terms compatible with essential features of all social roles.¹⁵

The several mechanisms of defense—turning against the self, projection, reaction formation, and the rest—are closely tied to the psychosexual stages. Each can be shown to represent an instrumental relation to one's self and others which reconciles some socially deviant desire with the particular type of dependency and obligation associated with one of the stages of socialization. I have described these connections elsewhere, and wish only to call attention to the fact that the defenses, like the types of character, are built upon the fundamental nature and basic vicissitudes of social commitments (37).

The defenses are, however, of special interest in a comparison of Mead and Freud. Each is a special instance of social control being imported as self-control. Each first exists as a relation between persons, becoming internalized in the course of learning to participate in such relations. In each case, the whole relationship, which includes the influences that actors exert on one another, is imported. Thus what are often called the "dynamics" of the relationship are internalized along with the actors concerned. We need not infer, as Freud often does, that these dynamics are somehow supplied by the central nervous system. The relations involved are instrumental, not neural, in character.

If we are right in saying that the defenses represent one set of fundamental varieties of self-control, they must, in Mead's theory, represent a fundamental categorization of the varieties of self-awareness. It is true that the defenses constitute only one such categorization, but it is one of special relevance for the study of social organization and social control. It provides a set of problems of description and explanation in which the joint powers of Meadian and Freudian thinking can be exploited.

CONCLUSION

I began this paper by observing that social psychologists often considered Mead to be theoretically fundamental but empirically unfruitful; Freud as empirically provocative but theoretically wrong. What does our survey of their work suggest concerning these judgments?

I believe there are three reasons for the current view of Mead. First, he provides a way of formulating important aspects of almost any problem we touch in social psychology, but does not suggest many problems for investigation. Second, certain of his most relevant premises are untestable. Third,

¹⁵ Early usage of psychoanalytic ideas by sociologists is surveyed in Burgess (6) and Eliot (7).

many of these features of his scheme which are testable do not fall within the social psychologist's purview.

Every problem of interest in social psychology requires that we make some judgments about the relation of mind to body and social organization. Unless this relation is one that sees these three as independent but interacting, our problem is destroyed before it is answered. Mead's version of functionalist psychology provides what we require, and does so more admirably than do other available schemes.

But, as Mead left it, his functionalism does little to specify systematic relations among body, mind, and social organization and the various forms of each. In short, he left us a general approach for the formulation of our problems, but did not suggest a wealth of problems at which we should look.

Second, Mead is peculiarly unfortunate in having proposed as a major social psychological premise a dictum that seems untestable, whatever its heuristic value. I have in mind his judgment that self-awareness and reflective thought are products of social interaction mediated by language signs and products of it alone. The only relevant test of this notion would require a population of biologically normal human adults who had managed, without undue trauma, to become knowledgeable about a differentiated environment, who had learned to employ certain common vocal gestures instrumentally but not reflexively, and who lacked all human contacts from birth to maturity. I believe it is time to label as irrelevant for Mead's premise all of the materials on feral men, infra-human primates, aphasics, schizophrenics, and children. None of these provides a reasonable test of Mead's idea. The accounts of ferals are of doubtful validity. The chimpanzees and gorillas lack human biology as well as symbols. It is as plausible to explain the aphasic's difficulty from the damage to his brain as from his conceptual disorders. Schizophrenia seems more a result of traumatic rearing than of miseducation in symbol usage. The very young child is biologically and experientially immature as well as unskilled in language.

Although I am disposed to believe that Mead's premise is sound, I am also disposed to believe that it cannot be demonstrated to be so. I would propose that whatever use we may make of Mead should not depend on the truth of this particular premise, and that we stop fruitless debate about its validity.

I suggested, third, that many of the testable features of Mead's scheme do not fall within the purview of social psychology. Such matters as the exploratory and impulsive character of the organism (41), the existence and nature of choices which are not guided self-consciously yet not distorted by repression (8), and the nature and growth of motives (26) are under inten-

sive investigation by experimental psychologists. In each case, the evidence strongly supports a functionalist interpretation. In no case, however, are variations in social organization, as such, of crucial theoretical importance. The problems addressed are those of psychology proper, not of social psychology.

Freud's present status in social psychology is almost the exact complement of Mead's. In its original form, Freud's theory of mind was unacceptable. Once we separate out its instinctive and Lamarckian features, however, we find two remaining clusters of ideas, each of which continues to be useful, each being supported by considerable evidence (26). The first cluster is not a theory of mind, it is a theory of neural and organic functioning without consideration for the organism's instrumental relations with its environment. This theory is of the source of conditions that precede, accompany, and follow the presence of mind and provide limits to gratification.

The second cluster of ideas consists of features of instrumental relations, of mind, especially significant for understanding the way in which the social environment limits gratification. These include superego, the types of character, the methods of defense, and the process of therapy. Each has proven surprisingly easy to rationalize by means of a functionalist approach, and each fits readily into our growing social psychological treatment of socialization and social control (28, 31, 42).

Freud was wrong because he failed to distinguish clearly between mind and intra-organic functioning and because he overgeneralized the values and experiences of his patients. Freud is provocative because he provides a rich set of differentiations readily interpretable as variations in mind which, in turn, are particularly significant consequences of social organization's impact on individuals.

We should not conclude, however, that Freud and Mead have left us a psychology completely adequate for our needs. There are many current developments that extend or modify their work. Parsons' categories of action, devised to bring greater coherence to our burgeoning knowledge, represent just an elaboration of an instrumental view of mind. So also do the rapidly growing studies of identity, of socialization, of semantic principles, and of interaction process. The explanation of social change is pressing hard upon functionalism's scant treatment of reinforcement and of the processes by which images are selected to guide the release of blocked impulses. The interpretation of personality adjustment in old age is demanding a more adequate picture of the limits to the place of mind in the total pattern of behavior. Investigators have been forced to develop the causal and functional laws so scarce in psychoanalysis. Given these developments, and many others, it is

unlikely that the ideas of Mead or Freud will soon be only curiosities in the museums of our discipline.

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Social Class and Childhood Personality¹

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INTRODUCTION

During the past twenty-five years there has been a great deal of interest in the relationship between social class and personality—particularly in the bearing of social class on the personality of the child and the relationship between social class and adult mental illness. Because of space and time limitations, this paper will concentrate on social class influences on childhood personality and will not be concerned with the literature on youth and adults. The product of this interest in social class influences on childhood personality has been numerous books, monographs, research articles and essays—often with contradictory emphases and conclusions depending on the convictions, theoretical orientations, and research styles of the authors.²

The theoretical basis for expecting a substantial relationship between social class and personality rests on three major assumptions upon which there seems to be widespread agreement among social scientists. The first is that in all societies some system of social stratification exists whereby the members of the society are differentiated into subgroups or classes which bear to one another a relationship of social inequality. It is further generally acknowledged that persons in the society can be more or less located in the stratification system in terms of the characteristic social roles they play. Consequently it is possible to infer, crudely at least, the social class position of most individuals in terms of readily ascertainable criteria. The particular criteria will be dependent on the culture of the society in question.³ There are rather wide differences among writers as to the origins of stratification, the functions of stratification, the criteria of social classes, the meaning of the term class,⁴ the

¹ This paper was presented at the Berkeley Conference on Personality in Childhood, arranged by the late Professor Harold E. Jones, on May 5, 1960. It was prepared while the writer was a Fellow at the Center for Advanced Study in the Behavioral Sciences.

² An extensive bibliography, consisting of 195 items, has been deposited with the American Documentation Institute Auxiliary Publications Project, Photo Duplication Service, Library of Congress, Washington 25, D. C. Order Document no. 6906 remitting \$1.75 for 35 mm microfilm or \$2.50 for photo copies. Advance payment is required. Make checks or money orders payable to: Chief, Photo Duplication Service, Library of Congress.

³ A number of books, summarizing contemporary theory and research in social stratification, have appeared in recent years. See, for example, references 2, 3, 15, 19, 22, and 26.

⁴ The writer is not convinced that social class is the best term for describing the socio-economic levels treated in most of the literature covered in this paper. Actually, the term social class implies much more than has been established concerning the existence of classes with distinctive boundaries and subcultures. What is meant operationally by social class

number of classes, the rigidity of any particular stratification system, and almost any other aspect of theory, substance, or measurement which could possibly be raised, but almost everyone seems agreed that some system of stratification based on social inequality is an inevitable product of organized group life. The empirical basis of this proposition is strong in that no society has yet been studied in which a stratification system, fulfilling at least the minimum requirements stated above, has not been found.

The second assumption is that the position of the child's family in the stratification system determines in considerable measure not only the social learning influences to which he will be subjected during the early period of his life, and in later life for that matter, but greatly affects also the access that he will have to certain opportunities that are socially defined as desirable. Certainly, there seems to be ample evidence that this is true even in societies in which the stratification structure is not particularly rigid or the differences between the social classes extreme. While many social scientists would deny that American society has fixed classes each with its own distinctive subculture, none would claim that the learning environment of the child whose family is highly placed in the stratification structure does not differ materially from that of the child whose social class position is low. Also it is readily apparent that the styles of life, the material comforts, the value systems and the instruction, both intentional and unintentional, which the child receives about the roles available to him in society differ depending on the social class position of his family. And finally even his treatment in the neighborhood, community and larger society will depend for some time, at least, on his social status origins.

The third assumption on which there is general agreement is that the early experiences of the individual will be of considerable importance in determining his later social behavior. To be sure, there is rather massive disagreement about the particular psychodynamics of the relationship between early experience and later behavior, the specific or patterned experiences which produce other patterns or traits of later personality, or even the critical periods in terms of days, months and years in which the individual is most susceptible to influence. However, these details and differences of theory and commitment have not led to any widespread rejection of the basic notion of the primary importance of early experiences in shaping later personality. The experimental evidence on animal behavior and the somewhat more inferential

in most studies is simply a convenient category of socioeconomic status. While the writer would prefer to use the more accurate term socioeconomic status or simply social status, he bows to the trend in the literature and will use the term social class in this paper except in referring to those studies where the authors have themselves used socioeconomic status or social status.

knowledge about human learning furnish the empirical foundation for this assumption.

On the basis of these assumptions, the reasonable expectation would be that some distinct personality traits, configurations, or types might be found which would differentiate the children of the several social classes, or at least that the incidence of certain personality characteristics would be different for the children of the various social classes. The results of research efforts to elucidate these relationships have been disappointing for a number of methodological and theoretical reasons. It would be impossible and is unnecessary to review each of the numerous writings which have direct bearing on the problem, but it does seem worthwhile to examine some of the most important of them to see if it is possible to reach any valid conclusions on the extent and nature of the relationship between social class and childhood personality, to point out some of the weaknesses of the research in the field, and to make some suggestions for future research. This is the purpose of the present paper.

AN EXAMINATION OF SELECTED STUDIES

A convenient point of departure might be to look at examples of studies which illustrate various approaches to the problem. As a minimum these would seem to include (a) work based primarily on typological and informal observational procedures, (b) those in which detailed observations on class-related child-training procedures have been made and personality characteristics inferred, observed or systematically assessed, and, finally (c) studies in which some measure of social class position has been related directly to some independent assessments of personality.

Perhaps the best-known example of the first type of study mentioned is Arnold Green's "The Middle-Class Male Child and Neurosis" (11) which was originally published in 1946 and has been republished in numerous collections of readings. Green, stimulated by the neo-Freudian writers Horney and Fromm, and on the basis of his recollections of his childhood and young adulthood in a Massachusetts industrial community of about 3,000 persons, delineated a set of social psychological conditions that he had observed in middle-class families which he believed predisposed middle-class male children to neurosis. He observed that the middle-class parent is caught up in a life-long struggle for improvement of personal position in the class structure. The father's work takes him away from the home and involves the manipulation of others around him to further his personal career. He is ambivalent toward his son because the child takes time, money and energy that could be used for the father's social advancement and also interferes with his role as a partner and companion to his wife. The mother, too, is ambivalent toward her child. He interferes with her career aspirations and her individual pleas-

ures. Also, he causes worries and demands great care and attention. Despite the socially structured ambivalence of both parents toward their son, they train him to love them for the care and sacrifices they have made for him and force him to feel lost without their love. Thus, the middle-class boy suffers "personality absorption" to such an extent that he cannot turn to others for genuine emotional satisfaction. Moreover, he is faced with the constant threat of withdrawal of parental love. Little wonder, then, that he feels small, insignificant, unworthy, inferior, helpless and anxious! He can never escape his parents' norms at home, in school or in his play groups—always he must try to live up to their high expectations of him, or he will lose their love. Thus, he lives "alone and afraid in a world he never made." The lower-class (Polish-American) child suffers no such fate. Although parental authority is often harsh and brutal, it is also casual and external to the "core of the self." The children avoid their parents, in fact have contempt for them and band together in common defense against their cruelty. Consequently the parents do not have the opportunity or the techniques to absorb the personalities of their children. Thus, the lower-class boys do not suffer from the guilt, anxiety and extreme sense of insecurity from which the middle-class boy suffers as a result of his extreme dependency on his parents.

This is possibly an all too brief portrayal of Green's argument, but it summarizes his main points. Although the paper purports to be based on careful observation, no indication is given about the number of observations made of the socialization practices of either lower- or middle-class families, nor is there any indication of the frequency of neurosis or neurotic behavior among either lower- or middle-class boys—much less any direct evidence on the incidence of neurotic behavior among those middle-class boys (or lower-class boys) who have, as against those who have not, been socialized in the "middle-class way." Consequently the article might well be dismissed as a provocative and speculative essay except for the fact that it has served as one of the principal supports for the currently widely-held stereotype of the neurotic middle-class child and has fostered the idea that the lower-class child in our culture is relatively less subject to neurotic tendencies and symptoms. It also illustrates something of the current state of the field in that a paper which is based essentially on speculation and retrospection should be widely accepted as portraying an accurate account of the influence of social class on childhood personality.

The second type of study is perhaps most conveniently illustrated by the research done by members of the Committee on Human Development at the University of Chicago and originally reported in 1948 in two articles, one by Allison Davis and Robert J. Havighurst (8),⁵ "Social Class and Color Differ-

⁵ See also reference 9.

ences in Child Rearing," and another by Martha C. Ericson (10), "Child Rearing and Social Status." These studies were the first to report systematic empirical findings indicating that child-rearing practices of middle-class parents differ significantly from those of lower-class families. The findings of the Davis-Havighurst study were based on interviews with 98 middle-class (48 white and 50 Negro) and 102 lower-class (52 white and 50 Negro) mothers and dealt with a wide variety of child-training questions and the mothers' expectations concerning their children. Perhaps the most important finding of the study was the restrictiveness of the middle-class mothers in the critical early training of the child. They were shown to be less likely to breast-feed, more likely to follow a strict nursing schedule, to restrict the child's sucking period, to wean earlier and more sharply, to begin bowel and bladder training earlier and to complete toilet training sooner than were lower-class mothers. In addition, they generally followed stricter regimes in other areas of behavior and expected their children to take responsibility for themselves earlier. From these results the inference was drawn that middle-class children encounter more frustration of their impulses and that this is likely to have serious consequences for their personalities. Their findings regarding the differences in nursing and toilet training between the middle and lower classes were widely heralded and served to strengthen the conviction, especially of psychoanalytically oriented workers in the field—particularly those at the forefront of the culture and personality movement—that the socialization of the middle-class child in America was producing neurotic middle-class children and adults.⁶ Davis and Havighurst themselves did not make this assertion. Their own conclusions were rather equivocal concerning the supposed consequences of these differences in training for the middle-class child. Actually, the inference they drew regarding personality effects was that the training influences to which middle-class children are subjected are likely to produce an orderly, conscientious, responsible, tame, but frustrated child. The only direct evidence they presented about the personalities of the children studied was that thumbsucking, which may be seen as an evidence of oral deprivation, and masturbation, which may indicate general frustration, are both much more frequently reported for middle-class than for lower-class children.

The findings of the Chicago group and the inferences made from their findings as to the personality consequences of class-related child-training practices were widely accepted and held sway without competition for some time. However, they were finally challenged by the results of two carefully designed empirical studies with quite different research objectives. The first of these was the attempt by the present writer to determine the consequences of a

⁶ For a summary and critique of personality and culture literature see references 14 and 18.

variety of infant-training practices on independently assessed childhood personality characteristics and the second was the careful study of patterns of child rearing made by a group of behavioral scientists at Harvard under the leadership of Robert R. Sears.

The study of infant training and personality, published in 1952, was based on interviews conducted in 1947 with the mothers of 165 rural Wisconsin children concerning the practices they followed in rearing their children and subsequently relating the data thus obtained to the personality characteristics of the same children as these were determined from scores on both paper-and-pencil and projective tests of personality and ratings of the children's behavior by their mothers and teachers.⁷ The specific infant-training practices studied were those most stressed in the psychoanalytic literature, including: feeding, weaning, nursing schedule, bowel training, bladder training and punishment for toilet accidents. These experiences were not found to be significantly related to childhood personality characteristics as assessed in the study. Moreover, two carefully constructed factor-weighted indexes measuring permissiveness in toilet training and feeding produced even less positive results.⁸ In all only 18 out of 460 relationships tested in the study were significant at the .05 level and, of these, seven were opposite from the predicted direction.⁹ These results, along with evidence from studies not so directly focused on the problem, tended to undermine the confidence of many who had made the inferential leap from class-determined early training practices to class-linked childhood personality characteristics and types.

Equally upsetting evidence came in 1954 with the publication of a preliminary report from the Harvard study (21) by Eleanor E. Maccoby and P. K. Gibbs on "Methods of Child Rearing in Two Social Classes," and later when the more complete report of the study (30) was published by Robert Sears, Eleanor Maccoby and Harry Levin in their well-known book, *Patterns of Child Rearing*. Their results, based upon careful interviews with 379 New England middle-class and lower-class mothers (labeled "upper-middle" and "upper-lower" by Maccoby and Gibbs), clearly indicated no differences in infant-feeding practices between the two social classes, more severity in toilet training in the lower-class families, less permissiveness in sex training in the lower-class families, more restriction of aggression toward parents and peers (and more punitiveness where such aggression took place) in lower-class families, greater imposition of restrictions and demands on the child in

⁷ For another paper dealing with the effects of feeding techniques on oral symptoms, see reference 36. Other papers reporting on theoretical, methodological and substantive aspects of this study include references 31, 33 and 37.

⁸ For the indexes, see (37), page 144.

⁹ A replication of the study in Ceylon (38) resulted in the same conclusions.

the lower-class family, more physical punishment, deprivation of privileges and ridicule by lower-class parents, but no differences between the two groups on isolation and withdrawal of love. Needless to say, these results were in important respects directly contradictory to the findings of the Chicago group and provided little factual basis for continued acceptance of the stereotyped version of the middle-class mother as a rigid, restrictive, demanding and punitive figure whose behavior can but result in frustrated, anxious, conforming and overly dependent children (30, Ch. 12). Neither was there any evidence whatever to support Green's contention about personality absorption of the child in the middle-class family or its supposed consequent—the neurotic middle-class child.

As might well be expected, the findings of the Harvard group provoked considerable debate and Havighurst and Davis did a comparison of the data of the two studies after adjusting to make the age groups more comparable, but they still found substantial and large differences between the results of the Chicago and Harvard studies (12). A number of other studies (16, 17, 20, 40) have appeared in recent years that generally confirm the findings of the Harvard group. Finally, Urie Bronfenbrenner (4), on the basis of an examination of a whole battery of studies, both published and unpublished, found a basis for explaining some of the differences, particularly in infant feeding and toilet training, in terms of a trend toward greater permissiveness in these areas on the part of lower-class mothers up to World War II but with a reversal since then, middle-class mothers subsequently becoming more permissive in infant training. The data gathered over the 25-year period on the training of the young child seem to him to show that middle-class mothers have been consistently more permissive towards the child's expressed needs and wishes, less likely to use physical punishment and more acceptant and equalitarian than have lower-class mothers. Finally, he sees indications that the gap between the social classes may be narrowing. While one might disagree with some of his interpretations and question some of the data on which his trends are based, it is clear from his review that in the present situation the evidence clearly supports the findings of the Harvard group and furnishes little basis for the belief that the training practices of middle-class parents are more likely than those of lower-class parents to produce neurotic personalities in their children.

One other important study which carries the analysis of class-related child rearing a step forward has recently been reported (1958) by Daniel R. Miller and Guy E. Swanson, in *The Changing American Parent* (23, 24). In their study of child rearing in Detroit, Michigan, they add to the stratification position represented by social class a second variable dealing with integrative position in the social structure which they have called "entrepreneurial-

bureaucratic integration." Families with entrepreneurial orientations are those in which the husband works in organizations that are relatively small in size, with a simple division of labor, have relatively small capitalization, and provide for mobility and income through individual risk-taking and competition. Families with bureaucratic orientations are those in which the father works in a large and complex organization employing many specialists, paying fixed wages or salaries for particular jobs, and, in place of reward for individual risk-taking, provides security in continuity of employment and income for those who conform with organizational demands. Miller and Swanson feel there is reason to believe that this aspect of status interpenetrates the family and influences child-rearing practices. Consequently, in their analysis they classify their families not only by social class but also by entrepreneurial-bureaucratic position. The addition of this new dimension of status produced results which were not nearly as clear-cut and definite as they had expected. In keeping with their predictions, entrepreneurial middle-class mothers were not less permissive than entrepreneurial lower-class mothers and there were no differences between bureaucratic lower-class and middle-class families in this regard. Their predictions that entrepreneurial middle-class mothers would be more likely to train their children in an active and manipulative view of the world was not supported. Moreover, entrepreneurial and bureaucratic lower-class mothers did not differ to any appreciable extent in the way they trained their children. If only the class differences are considered, their results are quite similar to those of the Harvard group. It seems quite probable that the relative failure of the new dimension to add much to the predictive power of social class was to some extent due to the inadequacy of their scheme for determining entrepreneurial-bureaucratic orientation.¹⁰ Consequently, in future studies, better categorization and assessment of this dimension may produce greater associations.¹¹ In any event the idea of introducing other dimensions of status than social class position seems to be a good one and should be tested in other studies.

¹⁰ Apparently the use of this variable was something of an afterthought and consequently its operational definition for purposes of the research had to be based on data available from the interview rather than what might have been more pertinent information. See (23), pp. 67-70.

¹¹ Possibly it would be a better test of the hypothesis to simply compare the personality characteristics, for a large number of cases, of children brought up in families more clearly representing the entrepreneurial and bureaucratic ends of the continuum, i.e., children of owner-operators of independent retail establishments vs. children of government clerks. It might even be more rewarding to drop the bureaucratic-entrepreneurial orientation entirely and to examine the influence of specific occupations on socialization norms and practices, on the assumption that occupations differ in the extent to which they interpenetrate family life and influence the behavior of members.

A third type of research bearing directly on the relationship between social class and personality involves the correlation between measures of socioeconomic status (henceforth referred to as SES) and children's scores on personality tests and is perhaps well illustrated by a study by the present writer and A. O. Haller, "Social Status and the Personality Adjustment of the Child" (34; see also 1, 5, 6). A comprehensive review of the studies in which SES had been measured objectively and correlated with independent assessments of the personality of the child indicated that middle-class children consistently made a better showing than lower-class children. For the most part the correlations were low or the differences were small and often there was no indication that the association was statistically significant, that sampling was adequate, that the tests of status and personality were dependable, or that variables known to be related to status or personality or both were controlled (34, pp. 114-115).

Consequently it was decided to make a rigorous test of the hypothesized relation between SES and personality using a design in which both variables were measured objectively and independently for a large sample (1,462) of grade-school children in a culturally homogeneous community with a fairly wide range of SES. Correlation analysis techniques were used to determine the relationship between SES, as measured by father's occupation and a rating of the prestige of the family in the community, and personality adjustment as indicated by a factor-weighted score on the California Test of Personality. The zero-order correlation coefficients between the two status measures and the personality scores were determined. Then the multiple correlation coefficient of the two status measures and personality score was computed, and, finally, the relationship was determined with sibling position, intelligence, and age controlled. The results indicated a low but significant association between status and measured personality (.16 for father's occupation and child's personality score, .23 for prestige position and child's personality score, .25 for the multiple correlation of the two status measures and child's personality score). The combined effect of the two status measures was not significantly reduced when the controls were introduced. The direction of the correlations indicated that the lower the SES of the child's family the less favorable his personality test score.

Certainly these results indicate that only a relatively small amount of the variance in measured personality found in this group of children can be accounted for by their SES. However, the test of the hypothesis was stringent and the correlations might well be higher in communities with more distinct stratification systems, and if more refined measures of status and personality were used. In any event the correlations, particularly since they are not markedly different from those reported by others who have followed similar

methods, should not be dismissed. They at least help to explain some of the variance in measured personality—an area in which little measured variance has been explained by other measured variables. However, the results do not provide much encouragement for the view that social class is a major determinant of childhood personality and they offer still another instance of evidence against the claim that middle-class children suffer greater personality maladjustment than lower-class children.

In an attempt to explore further the relationship between SES and personality, the writers (35) next did a factor analysis of the 30 personality test items which had been found to be most highly correlated with SES. The results of this analysis indicated that four factors explained approximately 90 per cent of the common variance among the items. These factors were tentatively identified as (a) *concern over status*, (b) *concern over achievement*, (c) *rejection of family*, and (d) *nervous symptoms*. Each factor was negatively correlated with SES, their respective correlations being $-.31$, $-.18$, $-.12$ and $-.26$, indicating that the lower the status of the child the greater the tendency to score high (unfavorably) on each of the factors. The intercorrelation between the factors ranges from $+.25$ to $+.59$. Thus, there seems to be a tendency for children who are concerned about their social status to worry about their achievements, to reject their families and to display nervous symptoms. The evidence from this study points to the fact that these characteristics are more common among lower- than higher-status children. Again the correlations between SES and the personality characteristics indicated by the factors, although statistically significant, are low and offer only limited support for the notion that the position of the child in the stratification system has bearing on his personality pattern. They are, however, suggestive of a line of attack on the problem which may be somewhat more rewarding than some of the approaches employed thus far.

CONCLUSIONS REGARDING SOCIAL CLASS AND CHILDHOOD PERSONALITY

On the basis of this brief review of studies of the bearing of social class on the personality of the child, the following conclusions seem justified:

First, there is a growing body of evidence from empirical studies of several types indicating a relatively low correlation between the position of the child in the stratification system (social class) and some aspects of personality, including measured personality adjustment. The relationship has not been shown to be nearly as close as might have been expected, but there is mounting evidence that at least some of the variance in childhood personality can be explained by the social status position of the child. Possibly when better measures are used the relationship will prove to be higher. The present crude

techniques of measuring both variables doubtless result in underestimation of the correlation.

Second, the direction of the relationships found offer absolutely no support for the notion that middle-class children more commonly exhibit neurotic personality traits than do children of lower-class origins. Indeed all of the empirical evidence points to the opposite conclusion.

Third, the studies of child rearing in relation to social class, made since the publication of the Chicago studies, have found fewer class-related differences in infant training than might have been expected and those differences that have been found tend to indicate greater permissiveness in feeding and toilet training on the part of middle-class mothers rather than lower-class mothers. The findings in relation to early childhood training indicate less impulse control, less punitiveness, less reliance on strict regime, less restrictiveness in sex behavior and less restriction on aggression—in other words, generally greater permissiveness on the part of middle-class mothers.

Fourth, empirical studies of the consequences of child training have given a great deal of attention to such aspects of infant discipline as manner of nursing, weaning, scheduling, bowel and bladder training, but have found very little or no relationship between these experiences and childhood personality traits and adjustment patterns. Much less attention has been given to the consequences of other aspects of child training, but some low correlations have been found between such factors as patterns of punishment, permissiveness for aggression and mother's affectional warmth for the child and such aspects of personality as feeding problems, dependency and aggression. Although these correlations explain only a small portion of the variance in childhood personality, they cannot be entirely dismissed and, to the extent that the child-training practices are class-linked, they must be credited with having some bearing on the relationship between social class and personality. Certainly, however, the empirical evidence does not permit any lavish claims regarding the influence of the child-training variables studied on the personality of the child.

Fifth, a final inescapable conclusion from reading these and other writings on social class and childhood personality is that, with a few notable exceptions, the level of research and theoretical sophistication in this area has been appallingly low. Some of the most influential work has had little or no acceptable empirical basis. The evidence upon which widely accepted claims have been founded is sometimes from samples that are so small or so clearly biased that no reliable conclusions could possibly be reached. In fact there is not a single study that can claim to be representative of the whole society or any region of the country and only a small handful are clearly representative of any definable social system. The statistical techniques in some of the studies

are clearly inappropriate for the data. The theoretical guide-lines for most of the studies are seldom specified and often are not even discernible. The chain of inference from theory to data, to conclusions, to wider generalizations is sometimes unclear and instances can be cited in which links in the chain are entirely missing. Great lack of conceptual clarity, particularly concerning the two principal variables, social class and personality, is generally apparent. Thus, statistical categories of socioeconomic status measured by crude techniques are treated as social classes in the broader meaning of that term, and inferences are drawn about sub-cultures, learning environments, value systems and other social class characteristics without the necessary empirical evidence of their existence. Likewise the term personality is used in a variety of ways but with little attention to definition and specification. Often inferences are made about deeper levels of personality from more or less surface variables. Because of these weaknesses in theory and method, more definitive conclusions about the relationship between social class and childhood personality must await better designed studies.

SUGGESTED DIRECTION FOR FUTURE RESEARCH

In the light of the present situation in the field and because of the basic importance of the problem, both from the theoretical and practical points of view, a few suggestions regarding future research may be helpful.

First, although the available evidence concerning the relationship between child training and personality does not provide much of a basis for explaining personality variation and despite the fact that the present evidence concerning the relationship between social class and child training does not seem to indicate a very close correspondence, further studies of social class and child rearing are desirable and necessary. With our present knowledge of sampling procedures, data-gathering methods, and analysis techniques, a carefully designed large-scale study using a sample of sufficient size to permit racial and ethnic breakdowns and other needed controls and concentrating on various aspects of child rearing ranging over the whole period of childhood is the indicated next step. In such a study additional attention should be given to assessment of the behavioral correlates of child-training practices and to appropriate delineation of larger personality configurations. Additional studies of small local communities with relatively narrow stratification systems or studies in larger communities with samples that are inadequate to represent the full range of the stratification system are not likely to add much to the knowledge already available from existing studies and could well be dispensed with.

Second, despite the fact that many studies have been made of the relationship between the SES and the measured personality of school children, there

is still need for a definitive study in a large community with a heterogeneous population. Since it would be an overwhelming task to map the actual class structure of such a community, several objective SES indicators might be employed singly and in combination, using modern multivariate statistical analysis techniques to determine their relationship to measured personality as indicated by personality test results. With large samples, a number of variables could be controlled and some definitive conclusion might be reached regarding the relationship between SES and childhood personality. Further analysis, following the general model used by Sewell and Haller in their most recent study of test items having high correlations with SES, could be done to determine the factors which account for variance in responses to personality tests.

Third, there is need for an intensive study of the relationship between social class and personality in some community or society in which a functionally existing class system with well established sub-cultures is present, if indeed such can be found. It is clear from the literature that classes in this sense have been assumed to be operative but in no case have they been properly delineated, validated and sampled in any study of social class and personality. Such a task would be a major undertaking and probably could be carried out only in a modest-sized community, but, if an appropriate community were found, this would provide a more critical test of the theoretical relationship between social class and personality than any yet attempted. Unless and until such a study is done, no one is really justified in implying that social class is more than a convenient statistical category in discussing its relation to personality.

Fourth, it now seems clear that scientific concern with the relation between social class and personality has perhaps been too much focused on global aspects of personality and possibly too much on early socialization. Therefore, it is suggested that the more promising direction for future research will come from a shift in emphasis, toward greater concern with those particular aspects of personality which are most likely to be directly influenced by the position of the child's family in the social stratification system, such as attitudes, values and aspirations, rather than with deeper personality characteristics. It also is suggested that instead of focusing so much attention on the very young child, more research should be done on older children and adolescents—on the assumption that whatever differences one's social class position makes to the above-mentioned aspects of personality are likely to be the product of later and more gradual socialization experiences rather than the more proximate effect of specific aspects of early experiences (39). Evidence already available from a number of studies seems to clearly indicate that adolescents' belief systems and values differ rather clearly in relation to their

social status positions: adolescents of lower-class backgrounds appear to have lower need-achievement, lower achievement values (27, 28), are much less likely to place high value on a college education, less frequently aspire to high level achievement in educational pursuits and occupational activities (13, 36) and are less willing to defer their gratifications than are middle- and upper-class adolescents (7, 29). Recent studies by the writer and others have shown that there is a marked negative correlation between the socioeconomic status and the educational and occupational aspirations of high school seniors and that relationship remains even when sex and intelligence are controlled (36, pp. 70-72). As yet unpublished, results from a more recent study which the writer is currently conducting on a statewide sample of Wisconsin high school seniors indicate that other variables such as parental pressure, rural-urban background, community and peer group influences may also be introduced without negating the influencing of SES on these aspirations. It may well be that it is precisely in the area of attitudes, values, and aspirations that social class influences are most pronounced. If so, it would be profitable to study children of different social-class backgrounds to determine when and how these characteristics of personality develop and how responsive they may be to other influences.

Fifth, it is suggested that it might be more revealing and more promising in terms of the knowledge that may be gained about social influences on personality to focus some attention on intraclass differences instead of being concerned exclusively with interclass variations. The data of most empirical studies of social class in relation to personality and related variables indicate the existence of considerable intraclass variation. Thus in the writer's studies of social status and personality, there were many children in each social status level who made favorable scores on the personality measures and some at each level who made unfavorable scores. Even in the study of differences in educational and occupational aspirations, where sizeable differences were found between SES groups, there were important differences within each status group (36). Obviously, it is not intelligence or sex which accounts for the within-class differences found in this study, because these variables have been controlled, but it may well be that family attitudes and values, peer-group influences, and community forces will be found to explain a sizeable portion of the variance. Another interesting series of questions suggested by these results is: What are the personality effects of having values and aspirations that are deviant from those of one's social class? Does it mean that the lower-class child has to reject the values of his family and neighborhood in order to be socially mobile? If so, what are the dimensions and what is the nature of the stress experienced by the upwardly mobile lower-class child and what personality consequences flow from such striving? If he is successful in his

mobility aspirations, will the lower-class child find it possible to internalize the values of his new status position or will he be constantly plagued by the conflict between his old values and the new? These are just a few of the kinds of questions that could be studied in relation to intraclass differences in personality.

Sixth, it is suggested that, as basic a variable as social class is for social behavior, there are other important aspects and dimensions of social structure that cut across the social stratification system which should not be neglected in the study of the personality development of the child.¹² Among the more important of these are the mobility and the occupational orientations of the family. In addition, there are other traditional social structure variables such as age, sex, family size, sibling position, race, ethnic background and religion which probably play a significant role but have been taken into account insufficiently in studies of social structure and childhood personality. Moreover, much theoretical and analytical work is needed on the possible influence of various combinations of social structure variables and their joint as well as independent influence on the personalities of children.

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¹² Some provocative suggestions along these lines are given in (25).

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The Choice of Variables in the Study of Socialization *

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Perhaps the greatest change that has occurred in the field of child development in the past 15 years has been the increasing emphasis on socialization. The change may be traced by comparing the more traditional textbooks with recent ones. The scholarly child psychology text by Munn (16), for example, does not bring up the topic of parent-child interaction until the 16th chapter, and here devotes only eight pages to a topic called "environmental influences and personality," a heading under which he presents all that the book has to say on "mothering," on Freudian theory of developmental stages, on ordinal position—in fact, on socialization in general. Contrast this with a book such as Watson's (23), in which more than half the book is devoted to a discussion of socialization theory and a detailed consideration of the impressive amounts of research that have recently been done on the subject.

The same increasing emphasis on socialization may be seen in the child-development journals. And, of course, the widespread research interest in this topic has led to the development of several research instruments for the measurement of parental attitudes and behavior. There are the Fels scales (2), developed during the 40's, for the rating of parent behavior; the parent interview schedule developed by Sears and his associates at Harvard and Stanford (21), the parent attitude scales developed by Shoben at U.S.C. (22), and the widely-used Parent Attitude Research Instrument scales developed at the National Institutes of Health by Schaeffer and Bell (20), to mention only a few. Each investigator, when he sat down to make a first draft of his rating scale or interview schedule or attitude scale items, had to ask himself the question: What shall I measure? What are the important variables in parental behavior that ought to make a difference in the development of the child? The process of selecting and defining variables is, of course, the very heart of theory-making. There are as many possible variables as there are ideas about what causes what in human development. I cannot attempt here to give any sort of roster of variables; the task would be too great and might not prove very useful. I simply want to point out some of the major classes of variables that have been used and give a little of the history of the reasons why we have chosen to measure these things and not others and perhaps point to a few ways in which we could clarify the meaning of the dimension we are using.

* This article is a composite of two papers, one delivered at the Berkeley Conference on Personality Development in Childhood in April, 1960, and the other delivered at the American Psychological Association Meeting in New York in August, 1961.

Let us start with the traditional child psychologist, with his interests in motor development, emotional development, intelligence, concept formation, and personality development, all grounded in traditional principles of learning and maturation. He may look upon the current work in socialization with a jaundiced eye and inquire what the excitement is all about. He may feel that he has actually been studying socialization for years without calling it by this name. He might put his question this way: If it is true that socialization is the process of transmitting culture from one generation to another, and that the child acquires the modes of behavior prescribed by his culture through the process of learning, then how is the study of socialization any different from the study of learning itself? One might reply that in socialization studies, we study not only the child as learner but the parent as teacher. But a skeptic might still wonder how much difference this actually makes. For example, laboratory studies of learning have demonstrated that behavior which is followed by reward will be strengthened, and its probability of recurrence will be increased. Now, if a student of socialization does a study of dependency, and discovers that parents who reward their children for dependency have more dependent children, has he really found out anything that we didn't know already?

In my opinion, it is valuable to carry out at the human level studies which attempt to employ the standard variables that have grown out of laboratory study on learning, where most of the work has been done on sub-human species. But, in the process of applying such variables to socialization studies, the variables almost perforce undergo certain modifications and elaborations, with the result that translating traditional behavior theory variables into the socialization setting sometimes results in the addition of something new, and the possibility of getting new kinds of principles.

Let me give an example. Suppose we wanted to study the effects of a particular schedule of reward. What do we mean by reward? The traditional approach to reward has been to produce a physiological drive, such as hunger or thirst, through deprivation; and then to reinforce the desired behavior by presenting a drive-relevant reinforcing stimulus. But even in fairly young children, a rapid development of complex motivation occurs, and this changes the nature of the reinforcements to which children will be responsive. B. F. Skinner encountered this fact when he was developing his teaching machines. The early models were devised so as to emit little pieces of chocolate candy whenever a child made the correct response. But it was soon evident that a child progressed through a series of arithmetic or spelling problems just as readily without the candy; in fact, the giving of candy sometimes disrupted the learning process. Skinner, therefore, abandoned the candy rewards, and the current models of his machine rely upon no other reward than the child's

interest in doing his work correctly—buttressed, no doubt, by a certain amount of pressure from the teacher and parents. This incident illustrates a major question about the definition of variables: what happens to the variable “amount of reward” when it is translated into situations of teacher-child, or parent-child, interaction? In modern societies, children’s physiological drives are regularly and quite fully satisfied and are seldom used as a basis for training. That is, most parents do not let the child get hungry, thirsty, wet, or overtired, and then make the satisfaction of these needs conditional on good behavior. Rather, the rewards used are money, a trip to the zoo, being allowed to stay up for a special TV program, etc. A gift of candy for some children becomes symbolic of affection instead of vice versa. Very commonly, behavior is reinforced simply through the giving of approval, affection, or attention. So the concept “reward,” when it refers to the rewards which parents use in socializing their children is not directly comparable to the concept as it was originally developed in studies of animal learning. Of course, it is not really a new idea to point out that different kinds of organisms are capable of being rewarded by different kinds of things. It is clear enough that there are as many kinds of rewards as there are distinguishable motives, and that both motives and rewards vary between species and within species. But the new idea that has been added in socialization studies is that there may be distinguishable *classes* of rewards which may have different effects. The primary distinction made in studies so far has been between material reward and praise. Material reward covers all instances of giving the child some object or privilege that he wants, conditional upon good behavior. Praise depends to some degree upon the previous establishment of a relationship between the socializing agent and the child, such that the approval of this particular adult is something the child wants. That is, the effectiveness of praise ought to depend upon the identity of the person doing the praising and upon this person’s being someone the child loves, fears, or must depend upon for the satisfaction of needs.

The same kind of differentiation of a variable has occurred with respect to punishment. Students of the socialization process have been working under the assumption that not all kinds of aversive events following a child’s act will have the same effect. The distinction most commonly made is that between physical punishment and so-called love-oriented discipline, or withdrawal of love. There are other categories of punishment, too, such as withdrawal of privileges and ridicule, which are less interesting than the first two because there are fewer hypotheses about their probable effects. Let us concentrate for a moment on the distinction between physical punishment and withdrawal of love. Physical punishment is easy enough to define, although in rating its frequency and severity, the researcher is always troubled about the problem

of how to weigh slaps and shakings in relation to formal spankings. More tricky by far is the matter of defining withdrawal of love. Sears and his associates (21) have defined it as any act or statement on the part of the parent that threatens the affectional bond between the parent and child. This would include the mother's turning her back on the child, refusing to speak to him or smile at him or be in the same room with him, saying she doesn't like him when he does disapproved things, etc. The system of classification of techniques of discipline presented by Beverly Allinsmith in her chapter in Miller and Swanson's book, *Inner Conflict and Defense* (1), similarly emphasizes the distinction between "psychological" and "corporal" punishment, but defines psychological discipline somewhat differently. This classification for Allinsmith includes manipulating the child by shaming the child, appealing to his pride or guilt, and expressing disappointment over his misdeeds. But there is another dimension considered in the rating: namely, the amount of emotional control the mother displays in administering her discipline. Thus, if a mother shouts angrily at the child, "I hate you for doing that," Allinsmith would *not* classify this as psychological discipline, while Sears *et al.* would. But the mother who says calmly and perhaps coldly, "Now, dear, you know I don't like little boys who do that," would be classified as using psychological discipline in both systems. The difference in these two classification systems stems in part from two different views of the nature of the process which gives psychological discipline its effect. Sears *et al.* view it as a technique which arouses the child's anxiety over whether he is loved and approved of, and thereby elicits efforts on the child's part to regain his parents' approval by conforming, apologizing, or making amends. Allinsmith, on the other hand, emphasizes two things: (a) the *modeling* function of discipline, pointing out that a mother who loses her temper at the same time she is trying to teach the child to control his, will have a child who will do as the mother *does* rather than as she *says*; and (b) the target the child chooses for the aggressive impulses aroused in him as a consequence of punishment. The reasoning here is that the openly angry mother becomes a more legitimate target for the child's counter-aggression. The distinction between the two definitions of the dimension is further brought out when we consider the kinds of findings reported in the studies using them: Sears *et al.* found that withdrawal of love was associated with high development of conscience, physical punishment with low; Allinsmith found that psychological discipline, as she defined it, was associated with *indirect* fantasy expressions of aggression in the children they studied, corporal punishment with *direct* expression of aggression. All this illustrates the fact that fairly subtle differences in the definition of a dimension can affect the nature of child behavior that can be predicted from it. But more importantly, both these studies illustrate the fact that when we at-

tempted to take over the variable "punishment" from the learning laboratories, we found it necessary to subdivide and differentiate the variable and gained predictive power by doing so.

I have been attempting to cite ways in which I think that socialization studies have improved upon some of the standard variables employed in laboratory studies. There are instances, alas, in which we have not taken note of the differences which exist between the laboratory and the standard socialization settings, and thus have failed to identify and make use of some potentially promising variables. For example, in laboratory studies, we can take it for granted that the experimenter is there during training sessions, administering either reinforcements or aversive stimuli in some orderly relationship to the subject's responses. In the parent-child relationship, the parent is by no means always functioning as a trainer, and parents differ greatly in the degree to which they do so. Some parents keep track quite continuously of what the child is doing, and engage in a constant flow of interaction, both verbal and non-verbal, with the child. Other parents, for a substantial portion of the time they are with their children, are bored, busy, withdrawn, intoxicated, watching television, or subject to some other state or activity which precludes their responding to the child unless he becomes very insistent. In such a household the children are, of course, in a very different learning situation than children growing up with more wholly attentive parents. I think the sheer amount of interaction may in some cases be a more important variable for predicting characteristics of the child than the nature of the interaction that does occur. Let me give an example. In a study Dr. Lucy Rau and I are now doing at Stanford, we have selected groups of children who show discrepancies in their intellectual abilities. That is, we have one group of children who are good at verbal tasks but poor at number, another group who are good at spatial tasks but poor at verbal, etc. One of our students, Mrs. Bing, has interviewed the mothers of the children, and has also conducted some observation sessions in which the mother presents achievement tasks to the child while the observer records the kind and amount of the mother's involvement with the child's work. Mrs. Bing has found that it is the *amount*, rather than the *kind*, of mother-child interaction that best predicts what the child's pattern of intellectual skills will be. That is, the mothers of the highly verbal children use more praise, but also more criticism, than do the mothers of equally bright children whose area of special skill is non-verbal. Their total level of interaction with the child is greater, and this interaction includes the administration of what we would regard as aversive stimuli as well as reinforcements. The variable "amount of interaction" emerged in our factor analysis of the scales in the *Patterns of Child Rearing* study (21)—we titled this variable "responsible child-rearing orientation" for lack of a better name, but we never

made much use of the variable because it did not fit in with the theoretical formulation of our study. But I suspect that for any future work in which we are trying to predict such things as the child's cognitive maturity level or his achievement motivation, we may find that this variable is a better predictor than the less global variables (such as amount of praise) that we have been relying on up till now.

So far, I have been discussing the process of translating variables from laboratory studies of learning to the socialization setting, and have pointed out that we have been successful in employing such variables as reward and punishment, but that in the process of using these variables, we have found useful ways of subdividing them. Let us consider the theoretical meaning of the elaborations of these variables that have occurred.

When we make the distinction between material reward and praise, and the distinction between love-oriented punishment and punishment that depends for its effect upon producing direct physical pain, we are really taking note of the fact that the effect of discipline, and in fact the very nature of the discipline that is possible to use with a child, depends upon the history of the relationship that has been developed between the child and the person who is training him. And here is a new class of variables that socialization studies have added to the list of variables derived from classical studies of learning. In laboratory studies of learning, it has not been found necessary (at least until very recently) to ask whether the experimental subject loved or hated the machine that was emitting pellets of food and drops of water, or whether the characteristics of the machine or person presenting the rewards made any difference in the effectiveness of the reinforcement. Socialization studies, on the other hand, have found the identity of the socializing agent, and certain of his personality characteristics, to be important.

The emphasis on the importance of the relationship between trainer and learner came, of course, out of psychodynamic theories of personality development. Learning theory and psychoanalytic theory differ, I think, with respect to what they believe the basic nature of the socialization process is. This is an oversimplification, but I believe it would be reasonably accurate to say that a learning theorist would regard socialization as a learning process in which certain actions of the child's are selected out by virtue of reinforcement, others tried and dropped because they are in some way punished or non-reinforced. The parents have a primary role in administering the rewards and punishments for the child's actions, although they do not necessarily do this deliberately and consciously as a teaching effort. And, of course, there are other sources of reward and punishment than the parents' reactions which will help to determine what behavior the child retains.

The psychanalytic approach, on the other hand, would emphasize not the

detailed learning of specific actions on the basis of their outcome, but the providing of conditions which will motivate the child to take on spontaneously the socialized behavior the parent wants him to have. The terms introjection, internalization, learning through role-playing, and identification have been used in this connection; they all refer to the child's tendency to copy, to take on as his own, the behavior, attitudes, and values of the significant people in his life, even when the socializing agents have not said "that's a good boy" or given him a piece of candy for performing these acts or holding these values. I will not go into the controversy concerning which so much has been written as to whether the child is more likely to identify with the person who is powerful and feared or with the person who is loved; nor will I discuss the several thoughtful efforts by personality theorists to reconcile the two points of view. The only important point for our consideration here is that the psychoanalytic view of socialization has led to an exploration of such variables as the warmth or hostility of the socializing agent toward the child.

There can be no doubt that measures of the warmth of the parent-child relationship have turned out to be enormously useful in socialization studies, in a number of ways. In some studies, warmth has been found to have a direct relationship to some dependent variable. For example, McCord and McCord (14) have found that warmth in fathers was associated with low crime rate in sons. In other studies, warmth has turned out to be a useful cross-cutting variable which interacts with other variables in such a way that other variables only begin to show their effects when the sample is first sub-divided into groups differing in parental warmth. For example, in the *Patterns of Child Rearing* study, Sears *et al.* (21) found that withdrawal of love is associated with rapid development of conscience, but only if this technique is employed by a warm mother; also that punishment for toilet accidents disrupts the toilet-training process, but that the greatest disruption occurs if punishment is administered by a cold mother.

Warmth also occupies a central role in socialization studies in its relationship to other measures of child-training variables. There have been, to my knowledge, three factor analyses carried out on sets of socialization variables. One of these was on the Fels parent behavior rating scales (18), one on the PARI (27), and one on the dimensions employed by Sears *et al.* in the *Patterns* study (21). In the latter two, warmth emerged as a fairly clear factor. In the first, there were two factors, one called "concern for the child" and the other called "parent-child harmony," which taken together are probably close to what is meant by warmth in the other two studies. It is clear, then, that both in terms of its predictive value for the child's behavior and its central place among the other interrelated child-training variables, warmth is a variable to be taken seriously. Why is it so important? I have already

pointed out why the psychodynamic theorists believe it to be so—because of its role in producing identification. But the laboratory learning theorists can acknowledge its importance for another very simple reason. Before a parent can socialize a child, he must have established a relationship with the child such that the child will stay in the vicinity of the parent and orient himself toward the parent. A warm parent keeps the child responsive to his directions by providing an atmosphere in which the child has continuous expectations that good things will happen to him if he stays near his parent and responds to his parent's wishes. Fear of punishment can also make the child attentive to the parent, of course, but it establishes as well the conflicting motivation to escape out of reach of the punisher.

I'm sure I needn't belabor any further the notion that warmth is an important variable. But to say this is not enough. We still are faced with considerable difficulty in definition. It has been the experience of a number of people working with child-training data that they find themselves able to make reliable distinctions between mothers they call warm and mothers they call cold, and they find it possible to train others to make similar distinctions, but find it difficult indeed to define exactly what cues they are using to make the rating.

I suspect one source of difficulty is that the behavior we look for as indicating warmth varies with the age of the child the mother is dealing with. When the child is an infant, we are likely to label a mother as warm if she gives a good deal of the contact comfort that Harlow (8) has described. As the child grows older, the part played by the giving of contact comfort in the total constellation of warmth undoubtedly declines. When a child is ten, a mother seldom expresses her warm feelings for him by holding him on her lap. Rather, they are more likely to be expressed by the mother showing interest in the child and what he is doing, by helping unconditionally when help is needed, by being cordial and relaxed. Now warmth as expressed this way is not the same thing as giving contact comfort, and it is not to be expected that the same individuals would necessarily be good at both. Those of you who have read Brody's fascinating, detailed descriptions of mothers' behavior toward their infants (4) will perhaps have noted that the mothers who gave effective contact comfort, in the sense of holding the child comfortably and close, stroking it occasionally, imparting some rocking motion, handling it skillfully and gently in the process of caring for the child—the women who could do all these things well were not necessarily the same women who expressed delight and pride in their children, who noticed their little accomplishments, or who looked upon their infants as individuals. We should therefore not be surprised if there are low correlations between a mother's warmth toward her infant and her warmth toward the same child when it is older. If a primary

ingredient of warmth is being able to gratify the child's needs unconditionally, and if the child's needs change from the infantile needs for being fed and being given contact comfort to the more mature needs for various kinds of ego support, then it is necessary for a mother to change considerably as her child changes, in order to be warm towards him at all ages. Some mothers make this change more easily than others. It is true that Schaeffer and Bayley (19), in their longitudinal study of a group of mothers, did find a substantial degree of continuity in the degree of warmth displayed by a given mother toward a given child as the child grew older. There were undoubtedly individual differences in the ways warmth was manifested, and in the appropriateness of a mother's particular style of warmth-giving to the needs of her child at each developmental stage.

From the standpoint of making use of the variable in research, it appears that we should recognize that measuring the mother's current warmth at the time the child is, say, in nursery school or in the primary grades may not be an especially good index of how warm she was to the child as an infant. Furthermore, her warmth in infancy might predict quite different characteristics of the child than her warmth in middle childhood. If there is any relation at all between nurturance to an infant and its later personality traits, infant nurturance ought to relate only to those aspects of personality that presumably have their foundation in infancy—such as Erikson's dimension of trust (6), or various aspects of orality. Achievement motivation, on the other hand, if it is related to the mother's warmth at all, ought to be related to measures of this variable taken when the child is older. A finding of Bronfenbrenner's (5) seems to support this point about the importance of warmth-giving being appropriate to the developmental level of the child. He was studying high-school-aged children and employed several variables relating to the kind and amount of affectionate interchange between these adolescents and their parents. He measured the parents' affection-giving (in the sense of direct demonstrativeness), use of affective rewards, nurturance, and affiliative companionship. Among these variables, it was only the last one, affiliative companionship, that correlated with the child's current level of responsibility taking. We can speculate that this particular aspect of warmth is the one that fits in much better with an adolescent's needs than either giving him kisses or peanut butter sandwiches. All this means that warmth has to be defined in terms of parental responsiveness to the changing needs of the child.

I have referred to socialization variables that came originally from laboratory studies of learning, and that have been adapted for use in studying the socialization process. I have also referred to variables that originated in psychodynamic thinking. There is a set of variables that is difficult to classify

in terms of these two theoretical systems; I am referring to the dimension "permissiveness vs. restrictiveness," which emerged in our factor analysis of the *Patterns* variables, and to the related dimension of "control vs. laissez-faire" which has come out of the factor analysis of the *PARI* scales. The theoretical status of these variables is confusing because they relate to both psychoanalytic and learning theory, but the predictions from the two theories as to the probable effects of "permissiveness" or "control" are sometimes quite different. To cite a familiar example, there is the issue of what ought to be the effects of permissive treatment of the infant's sucking responses. The question is complex, but a simplified version of the opposing positions would be this: the learning theorist would argue that if an infant is permitted extensive sucking, his sucking habit will be strengthened, and he will be more likely to suck his thumb, pencils, etc., at a later age. The psychodynamic theorist would argue that permitting extensive infantile sucking satisfies oral needs and reduces the likelihood of excessive oral behavior at a later age. The same kind of difference of opinion can be found concerning whether permissive treatment of a child's aggressive or dependent responses should increase or decrease those responses. Now, of course, the fact that different theories produce different predictions concerning the effects of a variable is no reason for abandoning the variable. On the contrary, it is cause for rejoicing, and we should by all means continue to use the variable so that we can get data which will bear upon the validity of the theories. The trouble is that when we arrive at the point of trying to get agreement on the interpretation of findings, it sometimes turns out that the two schools of thought did not mean the same thing by "permissiveness." If a study shows that the more permissive parents are toward their children's aggression the more aggressive the children become, the psychodynamic theorist may say, "Well, by permissiveness I didn't mean *license*; the child must have limits set for him but he must also be allowed to express his feelings." If, on the other hand, a study shows that children heavily punished for aggression are more aggressive on the playground, or prefer aggressive TV programs, the learning theorist may say, "Well, of course, if the parents' methods of stopping aggression are such as to provide additional instigation to aggression, then their non-permissiveness won't eliminate the behavior." We begin to see that there are some hidden meanings in such a term as "permissiveness" and that we are dealing with several dimensions. Continuing with the example of aggression, we can see that permissiveness for aggression could mean the following things:

1. The mother holds the attitude that aggression is an acceptable, even desirable, form of behavior.
2. The mother does not like aggressive behavior and expects to limit it in her children, but feels that it is natural and inevitable at certain ages and so does not react strongly when her young child displays anger. A related definition of permissiveness

would be pacing the demands for self-control placed upon the child to correspond with his developmental level.

3. The mother is not especially interested in the child or is otherwise occupied, and does not act to stop or prevent his aggression because she does not notice what he is doing.
4. The mother does not act early in a sequence of her child's aggressive behavior, but waits till the behavior has become fairly intense.

And at the other end of the scale, the effect of *non*-permissiveness ought to depend upon how the non-permitting is done—whether by punishment, by reinforcing alternative behavior, by environmental control that removes the instigations to undesired behavior, or some other means. The basic point I wish to emphasize is that I believe “permissiveness” is not a unitary variable, and that we need to work more directly with its components.

So far I have discussed several classes of variables: the ones translated as directly as possible from laboratory studies of learning (e.g., amount and kind of reward and punishment), and variables such as warmth and permissiveness of the socializing agent, which have their origins more in psychodynamic theories. There is another class of variables which has been emerging as more and more important, namely the “social structure” variables. These variables have their origin largely in sociological thinking. I do not have time to give them more than the most cursory attention, but I do not believe they can be omitted if we are to do any sort of justice to the scope of significant variables employed in current socialization studies. One has only to list a few findings which have come out of the investigation of social structure factors to see how essential it has become to take them into account. Here is a brief sampling of such findings:

1. With adolescents, parents are most strict with children who are of the same sex as the dominant parent (17).
2. A mother's use of strongly dominant child-rearing techniques (called “unqualified power assertion” in this study) is related to her husband's F score (authoritarian personality score), but not to her own (11).
3. A mother's behavior toward her children is more closely related to her husband's education than her own, and her behavior is more closely related to her husband's education than is *his* behavior to his own education. Thus it appears that it is the family's social status, as indicated by the husband's education, that influences the mother's socialization practices (5).
4. Sons are more intra-punitive if their mothers are primarily responsible for discipline than they are if their fathers are the primary disciplinarians (10).
5. Aspects of social organization such as whether residence is patrilocal, matrilocal, or neolocal, and whether marriage is polygamous or monogamous, determine such aspects of culture as the length of the postpartum sex taboo, the duration of exclusive mother-child sleeping arrangements, and the amount of authority the father has over the child; these factors in turn influence such socialization practices as the age of weaning, the severity of the socialization pressures which are directed toward breaking up the child's dependency upon the mother, and the existence and

nature of puberty rites at adolescence. These socialization practices then in their turn influence certain aspects of personality, including certain culturally established defense systems (24, 25, 26).

6. When offered a choice between a small piece of candy now vs. a large one later, children from father-present homes can postpone gratification more easily than children from father-absent homes (15).

These findings all represent efforts to put socialization practices into a cultural or social-structural context. In each case, socialization practices are regarded as a link in a several-step chain, and consideration is given to the factors which determine the socialization practices themselves, as well as to the effects these practices in their turn have upon the child. It is clear that the way parents treat their children will be a function of their relationship to each other (especially of the distribution of authority between them), of the place the family has in the status system of the society in which the family resides, of the society's kinship system, etc. Of course, not every student of socialization need concern himself with all the steps in the complex sequence; he may, and often does, select a set of socialization practices and relate them to the child's behavior without going back to the conditions which led to these practices. But he needs to be aware of the degree to which socialization practices are embedded in a cultural context, and even needs to be alert to the possibility that the "same" socialization practice may have different effects when it is part of different cultural settings. So far, few studies have been planned or analyzed with this possibility in mind, but it might be worth some empirical examination.

It is time to make explicit an assumption that has been implicit so far about the constancy of personality from one situation to another and from one time to another. When we select aspects of parental behavior to study, and try to relate these to measured characteristics of the child, we usually measure what we believe to be reasonably pervasive, reasonably enduring "traits" of the parent and child. Orville Brim (3) in a recent paper, has leveled a direct attack at the notion of trait constancy. He has asserted that there is no such thing as a "warm" person, nor an "aggressive" person, nor a "dependent" person, but that behavior is specific to roles. This would mean that the same individual may be aggressive with his subordinates and dependent toward his boss; that a child may be emotionally expressive with his same-sexed age mates, but not with his teachers or his parents. The question of exactly how general personality traits are, is, of course, a matter that personality theorists have struggled with for many years. But our view of this matter will have some bearing upon our selection and definition of socialization variables. For if a child's behavior is going to be entirely specific to roles, then there is no point in trying to predict any generalized traits in the child; rather, we should be looking for those aspects of the socialization situation that will determine what

behavior will be adopted by the child in each different role relationship in which he will find himself. If we wanted to find what socialization practices were associated with the child's becoming dominant or submissive, for example, we would have to study how his dominant behavior had been reacted to when he was playing with same-sexed siblings, and study this separately from the socialization of the same behavior when he was playing with opposite-sexed siblings. Only thus could we predict, according to Brim, how dominant he would be with other boys in the classroom; and we would have to make a separate prediction of his dominance with girls in the classroom. We have already been following Brim's advice, in essence, when we do studies in which we test how the child's behavior varies with the role characteristics of the person with whom he is interacting. A good example is Gewirtz' and Baer's study on the interaction between the sex of the experimenter and the effects of interrupted nurturance (7). But to follow Brim's point further, we would have to investigate the ways in which the child's behavior toward specific categories of "others" was conditioned by differential socialization in these role relationships.

I do not believe that either socialization or the child's reaction tendencies are as role-specific as Brim claims; but obviously role differentiation does occur, and he is quite right in calling our attention to the fact that, for some variables at least, we should be studying socialization separately within roles. Actually, role is only one aspect of situational variability; we have known ever since the days of Hartshorne and May (9) that trait behavior like "honesty" is situation-specific. They found, for example, that the child who will cheat on the playground is not necessarily the same child who will cheat in the classroom, and that cheating is a function of the specific task presented to the child. This means that, in studying the effects of socialization, we either have to abandon efforts to predict characteristics like "honesty" and attempt to study only those characteristics of the child that are at least somewhat constant across situations, or we have to choose socialization variables that are themselves much more situation-specific, and make much more detailed predictions. An example of the utility of making socialization variables more specific to the situations they are intended to predict is provided in a study by Levy (13), in which it was found that a child's adjustment to a hospital experience was *not* a function of the parents having trained the child generally to meet many different kinds of stress situations; rather, the child's response to hospitalization was predicted only from the amount of training the parent gave in advance for the meeting of this *particular* stress situation.

The same sort of situation prevails with respect to trait constancy over time. In their recent article on dependency, Kagan and Moss (12) were able

to present repeated measurements of dependency in the same group of individuals—measurements which began at the age of three and continued into the late twenties. The most notable feature of their findings was the absence of continuity in this trait. The children who were dependent at age three and four were not the same individuals who emerged as dependent in adulthood. There was simply no continuity at all for boys, while there was some, but not a great deal, for girls. Let us consider Kagan's findings from the standpoint of efforts to study the socialization practices that are related to dependency. The first and obvious point is that we cannot expect to find any characteristic of the parent's behavior that will correlate with dependency in the young child and also correlate with dependency when the child is an adolescent or adult. This is not to say that the only correlations we can hope for are those between socialization practices and child characteristics measured at the same point in time. It is of course most likely that we shall be able to find aspects of a parent's current behavior that correlate with characteristics his child is displaying at the same time. But it is also possible that we could find aspects of the parent's current behavior whose effects will not show up until later. That is, perhaps there were things the parents of Kagan's sample of children were doing when these children were three and four that had some bearing upon how dependent the children became at the age of ten or eleven. But it is clear enough that whatever these delayed-action variables are, they could hardly be the same variables as the ones which determined how dependent the children were at age three, since it was not the same children who were displaying large amounts of dependency behavior at the two ages.

I have pointed to the way in which different theoretical systems, and different social-science disciplines, have converged to define and elaborate some of the variables which have been used in studies of socialization. In some cases this convergence has produced useful new knowledge; in others it has produced confusion over the meaning of variables. More importantly, it has produced a startling range of findings which have not yet been integrated into a theory of socialization. This is a major task that remains to be done.

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Sex Differences in Identification Development¹

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The purpose of this paper is to contribute to the theoretical formulation of sex differences in the development of identification, and to review data relevant to this formulation. The concept of identification has held a prominent position in the behavioral sciences. Identification has not only been used in relation to a given sex role or to a parental role, but it has also been used to indicate the feeling of belonging to a group, one's solidarity or involvement with the group, and one's incorporation of the group's values and attitudes. Since the term identification does carry varied meanings, let us define identification as it is used here by first differentiating it from other related concepts. Brown (2) contrasted identification to *sex role preference*. Sex role preference refers to the desire to adopt the behavior associated with one sex or the perception of such behavior as preferable or more desirable. In a previous paper (20) this investigator suggested that identification can also be contrasted to *sex role adoption*. Sex role adoption refers to the actual adoption of behavior characteristic of one sex or the other, not simply the desire to adopt such behavior. The fact that a woman on appropriate occasions wears trousers or short hair does not necessarily mean that she is identified with the male role, even though she is adopting certain aspects characteristic of that role. *Sex role identification* is reserved to refer to the actual incorporation of the role of a given sex, and to the unconscious reactions characteristic of that role. The differentiation among these concepts is elaborated at greater length in this investigator's previous paper (20).

THEORETICAL FORMULATION

The present formulation differs from the classical Freudian position which postulates that the boy's anatomical advantage arouses intense resentment and envy in the girl, drives her in the direction of masculinity, and hence, makes the acceptance of femininity difficult (10). On the contrary, the position taken in this paper follows Mowrer (21), Parsons (23) and others in stressing that the early closeness of the girl to the same-sex parent (the mother) gives her an initial, if temporary, advantage in progressing toward appropriate identification. This initial advantage may be largely counterbalanced by later learning experiences in this masculine-oriented culture.

¹ The writer not only leaned heavily on publications by Dr. Daniel G. Brown, U. S. Air Force Academy, in Developing this theoretical formulation, but he also gained a great deal from discussions with him.

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It is postulated that both male and female infants learn to identify with the mother (or the person playing the mother-role). Learning to identify with the mother is among the individual's earliest learning experiences. One of the basic learning principles states that early learning has "primacy" over later learning, i.e., early learning is more easily reinforced, and weakens more slowly with time than later learning. Jost's laws can be referred to in this connection. These laws are translated from the German as follows:

First law: "Given two associations of the same strength, but of different ages, the older one has greater value on a new repetition."

Second law: "Given two associations of the same strength, but of different ages, the older falls off less rapidly in a given length of time." (15, p. 472)

Dollard and Miller (9) suggest that pre-verbal learning is often more resistant to modification than verbal learning. This suggests that learning acquired early in life may have primacy over later learning.

In the present formulation, one of the major sex differences in the development of sex role identification is postulated as follows: The boy must shift from his initial identification with the mother and achieve identification with the masculine role, whereas the girl need make no such shift. Since this early learned identification with the mother is accorded primacy over later learning, one would hypothesize that the shift from mother to masculine identification may be psychologically difficult for boys. Because of the difficulty in shifting identification for the boy, and the fact that the girl need make no such identification shift, it is predicted that a higher proportion of males than females will fail more or less completely to form a same-sex identification.

Although recognizing that all homosexuals are not inverted in identification, it is nevertheless assumed that the *relative* incidence of homosexuality for males and females reflects the *relative* incidence of individuals who more or less completely fail to form same-sex identification. Brown (3, 4) made the prediction that there would be a higher incidence of male than female inverts. Considering inversion to refer to the total personality structure, he predicted that most male and female inverts would be predominantly or exclusively homosexual. On the other hand, he did *not* predict that all homosexuals would be inverted, assuming rather many determinants of homosexuality other than inversion. However, if the proportion of inverts for male homosexuals is approximately the same as that for females, then the incidence of homosexuals in males and females would reflect the *relative* incidence of inversion for the two sexes. Acknowledging, of course, that the frequency of homosexuality in males and females is not *necessarily* a valid measure, it is nevertheless assumed a probable rough estimate of the relative incidence of inversion in the two sexes. Brown used the Kinsey reports to suggest whether or not the relative incidence of inversion differs in males and females. The Kinsey

studies (16, 17) show a greater frequency of homosexuality among males, consistent with the hypothesis that more males than females fail to achieve same-sex identification, but rather form an opposite-sex identification.

The literature contains seemingly contradictory findings, showing more adult males than females who state preference for being a member of their own sex (11, 12, 32) and a higher proportion of adult males than females who draw the same-sex figure first (6); while on the other hand, the data referred to above indicate that there are more male than female homosexuals (16, 17). If males prefer being male more than females prefer being female, and if males are better same-sex identified than females (assuming that drawing the same-sex figure first reflects appropriate identification), then why should there be more male than female homosexuals? In the present formulation, those males who fail in forming same-sex identification are, for whatever reasons, unable to overcome the primacy of the early learned identification with the mother and remain "fixated" at that level. Perhaps the development of masculine identification typically progresses satisfactorily if it "once gets off the ground," that is, if the initial step is taken in early life. If the boy fails to make progress in this initial shift from mother to masculine identification, he may be unable to profit by the elaborate system of reinforcements provided males by the culture in developing masculine identification. If the boy is sufficiently fixated in his identification with the mother, these reinforcements for males may not even seem, to the boy, to apply to him at all. The mother-identified boy may consider these reinforcements to apply only to "those other children" falling in the male sex category to which he, phenomenologically, does not belong.

Let us summarize the formulation thus far developed as follows:

Early learning has "primacy" over later learning, and mother identification is among the earliest learning experiences. Consequently it is predicted that shifting from mother to masculine identification may be difficult for many and impossible for some males. The girl, on the other hand, need make no such shift in identification. Because of the difficulty in shifting identification, it is predicted that a greater proportion of males than females become identification casualties. The hypothesis that emerges is that *more males than females fail more or less completely in achieving same-sex identification, but rather make an opposite-sex identification.*

Now to proceed with the formulation. Those boys who do manage the shift from mother to masculine identification discover that they do not belong to the same sex category to which the mother belongs, but rather to the sex category to which the father belongs. The young boy discovers that he is no longer almost completely in a woman's world characterized by the maternal care received during infancy, but is now increasingly in a man's world. The

boy is under considerable pressure to adopt the masculine role, to be a "little man." These demands are made on him despite the fact that he has fewer men than women models for identification. Not only are his teachers typically women, but, because his father works all day, he is separated from his father more than from his mother. Despite the shortage of male models, a somewhat stereotyped and conventional masculine role is nonetheless spelled out for him. Sherriffs and Jarrett (29) found that men and women share the same stereotypes about the two sexes.

The development of the appropriate sex role identification for the girl, in this formulation, is quite different from that for the boy. In many ways her development is the converse of that for the boy. When the girl leaves infancy, she goes from a woman's (same-sex) world of mother care to a man's (opposite-sex) world. As Brown (5) pointed out, "The superior position and privileged status of the male permeates nearly every aspect, minor and major, of our social life." In this connection Smith (30) found results to suggest that children, as they grow older, increasingly learn to give males prestige; and Kitay (18) found that women share with men the prejudice prevailing in our culture against their own sex.

The girl is affected by many cultural pressures despite the fact that she need not shift identification, and despite the physical presence of the mother during her development. In this formulation it is postulated that sex-role identification, being a learned phenomenon and following the laws of learning, will tend to become extinguished without adequate reinforcement. It is assumed that in our society there is a relative lack of rewards for being female, and it is predicted that the lack of rewards for being female tends slowly to extinguish the girl's early learned feminine identification. It is further predicted that the prestige and privileges offered males but not females, and the lack of punishment for adopting aspects of the masculine role, have a slow, corrosive, weakening effect on the girl's feminine identification.

Another factor which may contribute to the girl's weakening identification with the feminine role is the mother's ambivalence in her own feminine identification. The strength of the girl's identification with her own mother may, paradoxically, contribute to weakening the girl's feminine-role identification. As this formulation develops, the importance of clearly distinguishing between identification with one's specific parent and identification with a more general sex role will become apparent. The girl, in being closely identified with her mother, also identifies with her mother's ambivalent feminine identification.

Thus, for these various reasons, it is predicted that, with increasing age, the female becomes less firmly identified with the feminine role. Conversely, the prestige and privileges accorded the male, the rewards offered for adopt-

ing the masculine role, and the punishment for not doing so are predicted to have a gradual strengthening effect on the boy's masculine identification. Thus it is predicted that, with increasing age, boys become relatively more firmly identified with the masculine role and girls relatively less firmly identified with the feminine role. In the previous paper referred to above (20), this investigator reviewed literature which suggests the validity of this hypothesis, assuming that sex role identification is reflected in the figure drawn first when the child is requested to draw a person. Brown and Tolor (6) reviewed a number of studies on human figure drawings. The studies on figure drawings with children show that, with younger children, a higher proportion of girls than boys drew the same-sex figure first, and with older children this trend is reversed, and a larger proportion of boys than girls drew the same-sex figure first.

If one can assume that psychological disturbances are associated with inadequate same-sex identification, then it follows that a much higher proportion of boys than girls should develop psychological problems at a very early age, because young boys are postulated to be less firmly same-sex identified than young girls. However, it was also predicted that, with increasing age, males become relatively more firmly identified with the masculine role, and females relatively less firmly identified with the feminine role. Consequently, it is predicted that males, with increasing age, develop psychological disturbances at a more slowly accelerating rate than females.

Why, however, should one assume that psychological disturbances are associated with inadequate same-sex identification? The argument may be pursued as follows: To the extent that an individual is poorly identified with his own sex, he is what Brown (3) calls a "psychosomatic misfit." To the extent that same-sex identification is replaced by opposite-sex identification, the individual has the bodily characteristics of one sex and the psychological characteristics of the other. One would expect conflicts stemming from the obvious realities of one's anatomical classification and one's tendencies toward opposite-sex identification. Conflicts might also arise from the expectations of society and one's inadequate same-sex identification and/or one's tendencies toward opposite-sex identification. Anxiety would be expected to accompany such conflicts. Various defense mechanisms would develop to cope with the anxiety. The reader can easily pursue the argument for himself and recognize that psychological disturbances could logically follow. Mowrer (21) goes so far as to postulate specific kinds of psychological disturbances as associated with various stages of identification. According to Mowrer, identification with the same-sex parent results in normal adult sex-role behavior; identification with the opposite-sex parent results in inverted adult sex-role

behavior; and identification of a confused, ambivalent nature with both parents results in neurotic adult sex-role behavior.

Despite the theoretical rationale for the assumption that psychological disturbances should be associated with inadequate same-sex identification, the research in this area is meager, and the operational definitions of identification often differ, as do the criteria of psychological disturbance.

Several relevant studies considered perceived similarity between the subject and a parent as a measure of identification. Perceived similarity involves the subject's giving responses to one or another psychological scale in the usual way, and then responding to the same scale as he *thinks* his parent might. This criterion of parental identification does not satisfy the definition utilized in this paper, which would require the *actual* (not just perceived) similarity in reaction between a subject and his parent. Cowan (8) utilized the semantic differential (22) to obtain ratings by each subject of his actual self, ideal self, mother, and father. He found that normal eighth-grade public school males or females did not differ from emotionally disturbed ones in the degree of their perceived similarity with either parent. On the other hand, Sopchak (31), having subjects take the MMPI in the usual way, as their father would, and as their mother would, found that men with tendencies toward abnormality show greater lack of perceived similarity with fathers than with mothers, but they also fail to show perceived similarity with mothers. He also found that for both men and women, failure to "identify" with the father is more closely associated with trends toward abnormality than is failure to "identify" with the mother. Cava and Raush (7), in a study in which adolescent boys filled out the Strong Vocational Interest Blank in the usual way and as they thought their fathers would, found that those showing the least perceived similarity showed the most conflict in the Blacky Test, especially in the castration fear area. Schoeppe, Haggard, and Havighurst (27), who did not use perceived similarity as a measure of identification, but rather based their conclusions on a great volume of material obtained in an intensive interdisciplinary study of 16-year-old adolescents, found that for boys, but not for girls, success in achieving emotional independence and in performing sex-role tasks is associated with primary identification with the same-sex parent.

The research data, although inadequately testing the assumption, suggest a relationship between poor same-sex identification and psychological disturbance for males, but not for females. However, the data are inconclusive for both. More research concerning this crucial assumption, using more adequate criteria of identification, is badly needed.

It is not necessary for a relationship to exist between inadequate same-sex identification and psychological disturbances in *both* sexes to predict that with

increasing age males develop psychological disturbances at a more slowly accelerating rate than females. As long as it can be assumed that such a relationship holds for *either* males or females, the rate of psychological disturbances should still reflect the adequacy of same-sex identification in that sex. On the basis of the available research data, no relationship between same-sex identification and psychological disturbances can safely be assumed for the female. Therefore, the rate of psychological disturbances in the various age groups for females can serve as a standard against which to measure the changes with age in rate of disturbances for males.

Now let us state the hypothesis which is suggested from the above formulation. If psychological disturbances are associated with inadequate same-sex identification for males, and if young boys are less firmly same-sex identified than young girls but become more firmly same-sex identified with increasing age, then it follows that *with increasing age, males develop psychological disturbances at a more slowly accelerating rate than females.*

Let us now see how well this hypothesis is supported by data. It should be clearly recognized that these data do not adequately test the theoretical basis for this hypothesis, but, rather, are suggestive only. Since a relationship between psychological disturbance and same-sex identification is assumed for males, and since we assume that young boys are poorly same-sex identified, then it is predicted that a high proportion of boys develop psychological disturbances at a very early age. The hypothesis predicted that, with increasing age, males develop psychological disturbances at a more slowly accelerating rate than do females, for, as they become older, the males become relatively better same-sex identified.

If this hypothesis is valid, such a trend should be reflected in demographic data related to the frequency of psychological disturbances in the two sexes. This trend should be reflected in the data showing the relative frequency of referrals to child guidance clinics, and in admissions and residence in psychopathic hospitals and prolonged-care mental institutions. With increasing age males should use such facilities at a more slowly accelerating rate than females, the rate for the females being considered a standard against which to measure age changes in males. This is precisely what the demographic data show. Gilbert (13) did a survey of referral problems in metropolitan child guidance centers. Calculations on Gilbert's data show that at age six and below, 2.47 times as many boys as girls were referred to child guidance centers. At ages 14 through 17, only 2.16 times as many boys as girls were referred. However, the reader might raise the objection that this trend, assuming it is a stable one, can be accounted for by the fact that psychiatric referrals in early childhood are most likely to occur when there is a problem involving aggressive behavior. Since boys are more aggressive than girls (14,

19, 26, 28), this would account for more boys than girls being referred to child guidance clinics in the very early years. However, psychological problems other than those involving aggressive behavior will sooner or later manifest themselves, and for that reason the boys, with increasing age, are brought to child guidance clinics at a more slowly accelerating rate than are girls.

If this objection is valid, then a recalculation of Gilbert's data, leaving out of the calculation those cases falling into the category "aggressive and anti-social behavior," should *fail* to show boys, as they grow older, using child guidance clinics at a more slowly accelerating rate than girls. This investigator did recalculate Gilbert's data, deleting from the calculation those cases falling in the "aggressive and anti-social" category. He found that, even without this category, at age six and below, 2.36 times as many boys as girls were referred to child guidance centers. At age 14 through 17, 2.10 times as many boys as girls were referred. Thus, the trend persists even when the category for aggressive and anti-social behavior is deleted. This suggests that this trend cannot be accounted for simply by the more aggressive nature of boys.

Although these data are in the predicted direction, they are unimpressive in and of themselves. However, additional data from different kinds of psychiatric facilities show that this same trend continues into adulthood.

Data from U. S. Public Health publications (24, 25) show that this same trend persists into adulthood both in terms of first admissions and of patients in residence in public and private psychopathic hospitals and public prolonged-care institutions. Table 1 was taken from data in these U. S. Public Health publications. Table 1 shows that, with increasing age, through age category 25 through 34, males almost uniformly use these facilities at a more slowly accelerating rate than do females. Row 1, for example, indicates that there were 748 males and 456 females under 15 years of age reported as first admissions in public prolonged-care mental hospitals in the U. S. in 1955. This ratio is 1.64 males to 1 female. However, there were 5,615 male and 3,942 female first admissions in the age category 15-24. This ratio is only 1.42 males to 1 female. There were 9,403 male and 7,750 female first admissions in the age category 25-34. The ratio of males to females in this age category is only 1.21 to 1. Thus, with increasing age the male/female ratio decreases. The rest of the table can be read in the same way.

Perhaps only psychotic or neurotic patients would better represent psychological disturbances which should relate to inadequate identification. Rows 5, 6, 8, and 9 show that this trend holds not only for the total patient population, but that it holds also for the patients with psychotic disorders and those with psychoneurotic reactions alike. The only reversal in the trend is found in Row 9, for psychoneurotic reactions in private hospitals for the mentally ill. However, there were so few cases under 15 years of age that

TABLE 1
Patients in Public and Private Hospitals for the Mentally Ill, by Age and Sex, in the United States, 1955

	Ages (In Years)					
	Under 15			15-24		
	M	F	M/F Ratio	M	F	M/F Ratio
Public Hospitals:						
1. First Admissions in Public Prolonged-Care Hospitals	748	456	1.64	5,615	3,942	1.42
2. First Admissions in Psychopathic Hospitals	122	71	1.72	200	178	1.12
3. Resident in Prolonged-Care Hospitals	852	499	1.71	7,496	4,911	1.53
4. Resident in Psychopathic Hospitals	108	53	2.04	53	65	.81
5. Psychotics, Resident in Prolonged-Care Hospitals	172	110	1.56	3,515	2,753	1.28
6. Psychoneurotics, Resident in Prolonged-Care Hospitals	13	9	1.44	90	113	.80
Private Hospitals:						
7. First Admissions, Hospitals for Mentally Ill	128	147	.87	1,278	2,097	.61
8. Psychotics, Hospitals for Mentally Ill	30	36	.83	763	1,286	.59
9. Psychoneurotics, Hospitals for Mentally Ill	7	25	.28	152	396	.38
Note—M represents male and F Female.						
						Source: References (24, 25).

the data in this age range are probably unreliable. Thus, the hypothesis that, with increasing age, males develop psychological disturbances at a more slowly accelerating rate than do females was almost uniformly supported by the available demographic data.³ Despite the fact that this trend is so uniformly found, one should be cautious in placing too much confidence in such demographic data, where so many unknown variables may be operating.

SUMMARY

The purpose of this paper is to contribute to the theoretical formulation of sex differences in the development of identification, and review studies relevant to this formulation.

Both male and female infants were hypothesized to learn to identify with the mother. This early learned identification was hypothesized to have "primacy" over later learning. Boys, but not girls, must shift from the initial identification with the mother to masculine identification. If, for whatever set of circumstances, this shift is not undertaken at that time, it was postulated that it is extremely difficult, if not impossible, to make an adequate masculine identification. For those boys who make a successful identification shift during infancy, society spells out a stereotyped masculine role, and, despite a shortage of male models, they advance toward identification with this stereotyped masculine role.

Despite the fact that the girl need not shift her identification, and despite the physical presence of the mother during her development, the girl is still affected by many cultural pressures. The prestige and privileges offered males, but not females, and the lack of punishment for adopting aspects of the masculine role are predicted to have a gradual weakening effect on the girl's feminine identification. Conversely, the prestige and privileges accorded the male, and the punishment for not adopting the male role are predicted slowly to strengthen the boy's masculine identification.

From these considerations, the following hypotheses emerged:

1. More males than females fail more or less completely in achieving same-sex identification, but rather make an opposite-sex identification.
2. With increasing age, males develop psychological disturbances at a more slowly accelerating rate than females.

These hypotheses seemed to be generally supported by available data.

³ It is interesting to note that Table 1 shows a far higher proportion of females to males in the private hospitals than in the public hospitals. It is beyond the scope of this paper to attempt an explanation for this phenomenon. The crucial factor for this hypothesis is not the ratio of men to women *per se*, but rather that, with increasing age, males use these psychiatric facilities at a more slowly accelerating rate than females.

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The Diffusion of an Innovation Among Physicians in a Large City

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This is a report on the extent to which sociometric and other interpersonal dimensions appear to be significantly related to the diffusion of an innovation among physicians in a large city. Studies by others have suggested the importance of interpersonal dimensions. A pilot study in a New England community of some 30,000 (1) and a field study in four Midwestern cities ranging in population from 30,000 to 110,000 concluded that the degree of a physician's integration among his colleagues was positively related to his acceptance of the innovation represented by a new proprietary prescription drug (2). These findings have been made part of the basis for a contribution to the theory of innovations (3), and have provided evidence for the influence of the group in the adoption of new professional practices (4).

Generalizability in social research is of continuing concern to the researcher. Of how large a proportion of physicians are these findings likely to be true? Communities with a population of under 100,000, as of 1950, accounted for 34.6 per cent of the United States population. Communities of this size may account for perhaps half of the country's practising physicians. It would thus appear that the communities of the type in which the earlier studies were conducted may not include the majority of physicians. To establish the salience of the dimension of interpersonal contact among physicians in a community of different size, a related situation was studied in a large American city.

Any new drugs of the kind studied in earlier researches and in this study face substantial competition from established drugs as well as from the continuing flow of new products vying for the physician's attention. Some 500 new prescription products are introduced a year, of which an average of 400 will fail to win adoption, 50 will be somewhat successful and perhaps 50 will be successful. Summarizing a number of marketing-research studies on American physicians, we can say that the typical physician is informed about new drug products by articles and advertisements in medical journals (with which he spends an average of 4½ hours weekly), visits by detail men (with whom he spends an average of five minutes each), direct mail (an average of 75 pieces a week), exhibits at conventions and meetings, hospitals, samples and colleagues. Any new drug thus is likely to have to compete for the physician's attention with many other new drugs. For a physician to be aware of the existence and qualities of a new drug and for him to prescribe it means that it has managed to distinguish itself from the many conflicting and competing

claims for his attention. This study was designed to explore the correlation between interpersonal networks and knowledge of and prescription activity with a new drug, in view of the suggestions from earlier studies that the interpersonal network represented one possibly important way for a drug to come to a physician's attention.

METHOD

This study was conducted in a large city with a population of over three-quarters of a million. It has a number of hospitals and considerable facilities for medical education, both undergraduate and graduate. The method used was the personal interview. Every third physician in the community was selected for interviewing, by selecting every third name in a medical directory. Of the 933 physicians selected, it was possible to interview 816.

The physicians were interviewed in their offices after arrangements to do so were made by telephone. The interviews were conducted in late 1958 and early 1959 and dealt with the physicians' interpersonal network and with their knowledge of and prescription practices with respect to a new prescription drug which had been introduced late in 1957 and which will be called Chemneo in this report. The drug had been given much publicity and advertising support by its manufacturer. Chemneo was not the first of a completely new family of drugs, e.g., penicillin, but was a new and relatively unusual addition to a growing family of drugs which had recently been achieving great prominence. The drug was a definite innovation and had possible relevance for general practitioners as well as practically all of the specialties.

The specialty breakdown of the physicians interviewed was; general practitioners, 41 per cent; surgeons, 17 per cent; internists, 12 per cent; obstetricians-gynecologists, 9 per cent; pediatricians, 6 per cent; psychiatrists-neurologists, 4 per cent; ophthalmologists and eye-ear-nose-throat, 3 per cent; others, 8 per cent. Data on the physician's specialty status was obtained by direct questioning.

In addition to information on his career line, the physician was asked the three sociometric questions which had been asked on the earlier studies: To whom did he most often turn for advice and information? With whom did he most often discuss his cases in the course of an ordinary week? Who were the friends, among his colleagues, whom he saw most often socially? The names of three physicians were requested in response to each of these questions. The physician was also asked if he had heard of Chemneo, if he had ever prescribed it and, if so, when he had first prescribed it.

The data for this study thus consist of background and sociometric information on a sample of the physicians in the city, along with their awareness of and prescription activity with the recently introduced Chemneo.

RESULTS

The previous studies had found that the first networks of physicians to be operative as chains of influence were those of advisors and discussion partners, after which the friendship network became influential in the introduction and acceptance of a new drug. Although the physicians interviewed in previous studies apparently had little difficulty in naming three physicians in response to each of the three sociometric questions, the physicians in the current study had some difficulties in doing so.

Advice and Information

The majority of physicians interviewed were unable to name three physicians to whom they turned "most often" for advice and information. The average number of physicians named by each respondent was 1.9. The typical respondent observed that he did not call on a small group of physicians for either advice or information. Some physicians said that there were a number of colleagues in the community on whom they might call for advice or information, but which of these they might call would depend on the reasons for their seeking assistance, so that their calling would be for a relatively specific purpose rather than a habitual activity.

Physicians who had institutional affiliations tended to name the chief of their institution or department as the person on whom they would call. A few physicians interpreted the questions as an implied insult on their professional capacities. General practitioners were more likely to name a physician from whom they obtained advice and information than were specialists. A number of the specialists interviewed said that they had had exceptionally close relationships with their preceptors during the period of their residency, but when they became functioning specialists they had little need to seek the counsel of the former mentors. A few specialists who had been subordinates to various noted specialists said that their contracts with the latter required them to practice in a community other than the one in which their mentor was practicing, so that the mentors were some distance away and not easily accessible for advice and information.

Discuss Cases

When asked with whom they most often discussed their cases in the course of an ordinary week, a number of physicians had difficulties in responding to the question and in naming three physicians. The average number of physicians named by each respondent was 2.4.

The majority of the physicians named were those with whom the respondents had fairly regular and formal occasion to communicate about patients, rather than physicians with whom they developed opportunities for doing

so. A general practitioner might thus regularly talk to a gynecologist to whom he made referrals about the patient whom he was referring. A staff member of a medical group might talk to another staff member on an administrative matter connected with a patient who was of concern to both physicians. But spontaneous discussion of patients in a context of sociability, with a number of the same physicians, did not seem to occur often.

A number of physicians mentioned that their spouses were the persons with whom they were most likely to discuss a case. A number also said that they tended to avoid discussing cases with other physicians "because we don't talk shop." The younger physicians were more likely to mention physicians they talked to about patients than were the older respondents. Those in private practice were far less likely to name other physicians in response to this question, than were physicians in institutional contexts.

Friends Seen Socially

Practically every physician interviewed was able to name three physicians whom he saw socially. The average number named by each respondent was 2.9. The majority of those named were classmates of the respondent at medical school, or other physicians at generally the same stage on the career cycle as the respondent and thus roughly contemporary in age. Many lived at some distance from each other, although almost all were within the limits of the area studied.

Use of Chemneo

The dependent variable in this study was whether or not the physician had heard of Chemneo, whether or not he prescribed it for his patients, and when he had first prescribed it.

Three-fourths of the 816 physicians interviewed in the original sample had heard of the drug. Since the drug had been on the market for a year at the time the interviewing took place, the extent to which information about the drug had penetrated was thus fairly high. Three-tenths of the respondents were prescribing it by the time of the interviews.

Interacting Physicians

A total of 5,874 separate mentions were made of physicians in response to the three sociometric questions. Many of them were of the same physicians, either because the respondent named the same individual(s) in response to two or to all three questions, or because different physicians named the same person as the one(s) from whom they obtained advice and information, with whom they discussed cases and/or whom they saw socially. The total number of different physicians' names in the sociometric choices was 1,466. Five

hundred and thirty-five (37 per cent) of these sociometrically selected physicians were in the sample which was personally interviewed, so that their response to Chemneo was known. In the case of the other 911 physicians, data on their knowledge of and use of Chemneo were sought by telephone interview; 799 could be reached and were interviewed.

It was thus possible to compare the awareness of and prescription of Chemneo of the total population of physicians identified as involved in social interaction of several kinds (advice and information, discuss cases, friends). The total population could be divided into five groups on each of the three criteria of choice; physicians in the original sample who were not nominated,

TABLE 1

Knowledge of and Prescription of a Drug on the Part of a Sample of Physicians in Terms of Their Identification as Advisers

	Sample Members Not Nominated (N=342)	Sample Mem- bers Nominated Once or Twice (N=211)	Sample Members Nomi- nated Three or More Times (N=64)	Non- Sample Mem- bers Nominated Once or Twice (N=508)	Non-Sample Members Nomi- nated Three or More Times (N=143)
Proportion Who Heard of Drug, in Per Cent	74	76	75	75	75
Proportion Who Were Prescribing Drug at Time of Interview in Per Cent	29	32	31	28	30

members of the sample nominated once or twice, those in the sample nominated three or more times, physicians not in the sample who were nominated once or twice, and those not in the sample who were nominated three or more times. Table 1 gives the proportion of physicians in each of these groups who had heard of the drug and the proportion who were prescribing the drug at the time of the interview, in terms of the criterion of being nominated as a person to whom others turned for advice and information.

The data on Table 1 suggest that whether or not a physician was nominated as a person who gave advice and information and the number of times that he was nominated was not significantly related either to his having heard of Chemneo or to his prescribing it, as of the time of the interview. Table 2 gives comparable data on physicians (a) nominated as those with whom the respondent most often discussed his cases in the course of an ordinary week, and (b) nominated as friends seen most often socially.

Table 2 suggests that being identified as a discussion partner or as a friend,

and frequency of such identification, did not significantly affect whether the physician had heard of the drug or was prescribing it at the time of the interview. On all three measures of social integration, there thus appears to be no significant difference among the more integrated physicians who were nominated three or more times, the somewhat integrated physicians who were

TABLE 2

Knowledge of and Prescription of a Drug on the Part of a Sample of Physicians in Terms of Their Identification as Discussion Partners or as Friends Seen Socially

	A. Nomination as Discussion Partner				
	Sample Members Not Nominated (N=364)	Sample Mem- bers Nominated Once or Twice (N=222)	Sample Members Nomi- nated Three or More Times (N=78)	Non- Sample Mem- bers Nominated Once or Twice (N=555)	Non-Sample Members Nomi- nated Three or More Times (N=182)
Proportion Who Heard of Drug, in Per Cent	73	74	75	75	76
Proportion Who Were Prescribing Drug at Time of Interview, in Per Cent	30	32	31	30	29
	B. Nomination as Friends Seen Socially				
	(N=296)	(N=270)	(N=120)	(N=562)	(N=197)
Proportion Who Heard of Drug, in Per Cent	73	74	75 [†]	72	74
Proportion Who Were Prescribing Drug at Time of Interview, in Per Cent	30	28	31	32	30

nominated once or twice, and the isolates who were not nominated at all, in terms of whether they had heard of Chemneo or were prescribing it.

One of the earlier studies had suggested that there were four groups of physicians: innovators, influentials, followers, and diehards (1). Innovators who tried the drug first tended to be sociometric isolates while influentials who tried the drug next tended to be physicians with whom others interacted. Another study reported that the degree of a physician's integration among his local colleagues was strongly and positively related to the date of his first use of a new drug (2). In order to explore more precisely the measure of

number of months till first prescription, physicians in the current study were asked to attempt to recall when they had first prescribed the new drug. The physicians' responses were coded in terms of time samples in each of the five categories of nomination, in order to permit examination of the extent to which degree of social integration was related to date of adoption of the new drug. Table 3 gives the proportion of physicians first prescribing Chemneo at various time intervals in terms of their nomination as persons to whom the respondent turned for advice and information.

TABLE 3

Date of Initial Prescription of a Drug on the Part of a Sample of Physicians in Terms of Their Identification as Advisers, in Per Cent

Time Period Within Which Drug Was First Prescribed	Sample Members Not Nominated (N=342)	Sample Members Nominated Once or Twice (N=211)	Sample Members Nominated Three or More Times (N=64)	Non-Sample Members Nominated Once or Twice (N=508)	Non-Sample Members Nominated Three or More Times (N=143)
Within 2 Months					
After Introduction	.01	.02	.01	.02	.01
2-4 Months					
After Introduction	.03	.05	.07	.03	.05
4-6 Months					
After Introduction	.07	.09	.07	.08	.07
6-8 Months					
After Introduction	.11	.10	.09	.10	.09
8-10 Months					
After Introduction	.05	.06	.05	.07	.06
Never Adopted	.73	.68	.71	.70	.72

There appears to be no significant relationship between nomination as an adviser, or frequency of nomination, and date of first prescription of the drug. Table 4 presents comparable data on the dimension of being nominated as a person with whom the respondent most often discussed his cases in the course of a typical week, and on being nominated as a friend seen socially.

Nomination as a discussion partner or as a friend does not appear to be significantly related to when a physician first began prescribing Chemneo, or to whether he began prescribing it at all. The physicians nominated in each of the three choice situations did not adopt it before those not nominated, and incidence of nomination did not appear to be a significant variable. The rate of adoption of Chemneo of the sociometrically chosen physicians did not accelerate at any point. The rate of adoption appeared to be relatively similar in all groups studied.

TABLE 4

Date of Initial Prescription of a Drug on the Part of a Sample of Physicians in Terms of Their Identification as Discussion Partners or as Friends Seen Socially, in Per Cent

Time Period Within Which Drug Was First Prescribed	A. Nomination as Discussion Partners				
	Sample Members Not Nominated (N=364)	Sample Members Nominated Once or Twice (N=222)	Sample Members Nominated Three or More Times (N=78)	Non-Sample Members Nominated Once or Twice (N=555)	Non-Sample Members Nominated Three or More Times (N=182)
2-4 Months					
After Introduction	.04	.07	.07	.03	.06
4-8 Months					
After Introduction	.17	.19	.13	.18	.20
8-10 Months					
After Introduction	.06	.06	.06	.04	.05
Never Adopted	.73	.68	.74	.75	.69
	B. Nomination as Friends Seen Socially				
	(N=296)	(N=270)	(N=120)	(N=562)	(N=197)
0-4 Months					
After Introduction	.08	.05	.07	.07	.04
4-8 Months					
After Introduction	.18	.18	.18	.15	.19
8-10 Months					
After Introduction	.05	.08	.05	.05	.05
Never Adopted	.69	.69	.70	.74	.72

The differences between the results of this study and earlier studies are clear. One obvious consideration in any attempt to clarify the nature of these differences is the extent to which the level of response to the sociometric questions differs in the earlier and the current study. The subjects in the current study had some difficulty in identifying three physicians in response to the three sociometric questions. The question that they answered most completely was the one on friendship. We can compare the proportion of respondents not nominated as friends who are most often seen socially, the proportion of those nominated once or twice, and the proportion of those nominated three or more times, in the physician population in the earlier study (2) and in the present study. Table 5 gives the results of such a comparison.

Two out of the three comparisons were significant, suggesting that the incidence of choice in two of the three sociometric questions differs significantly in the two populations. This conclusion is not clear-cut, however, because 85 per cent of the total number of physicians in the relevant specialties in the

TABLE 5

Significance of the Differences Between the Proportion of Physicians in Each of Three Sociometric Friendship Categories in Earlier and Current Study

Sociometric Group	Earlier Study (N=125)	Current Study (N=1445)	Critical Ratio
Per Cent Not Nominated	28.8	20.5	1.99 ^a
Per Cent Nominated Once or Twice	44.8	57.6	2.79 ^b
Per Cent Nominated Three or More Times	26.4	21.9	1.10

^a Significant the .05 level.

^b Significant the .01 level.

four communities in the earlier study were interviewed and thus were rating each other to a great extent. In contrast, in the current study, one-third of the physician population in the city studied was interviewed, and could and did nominate physicians other than those in the sample that was initially interviewed.

This difference between the universe of physicians nominated in the earlier and in the current study is one of several differences in the design of the two studies. Another difference is that the current study uses subjective recall to establish data on drug adoption rather than check of prescription files. The current study treats sociometric nominations as an attribute rather than a variable. Chemneo was less successful than Gammanym, the drug studied in the earlier study, and it did not achieve as substantial a penetration of the market as did Gammanym. Chemneo may also be a drug of a different class than Gammanym.

It is possible that all those physicians who had adopted Chemneo are innovators. It is also possible that a drug which does not find universal acceptance follows a different diffusion path from a drug which does find universal acceptance, like Gammanym. This different diffusion path may be followed in both large and small cities.

Another possibility is that the diffusion paths followed by an innovation among physicians in smaller cities are different from those among physicians in large urban centers. Our respondents' comments suggest some speculations on how physicians in a large urban center may differ from those in smaller communities. Physicians in smaller towns are likely to know each other and to use the same hospital facilities. In contrast, physicians in a big city may be less inbred and may conduct their practice without regular contact with a small core of other physicians with whom they discuss cases and from whom they obtain advice. They are likely to have attending privileges at different

hospitals. Typical physicians in the city studied seem, by and large, to engage in relatively little discussion with other physicians and not to seek much advice from others. They may look a case up in the medical literature, or, if they feel that another opinion is useful, they may refer a patient to a colleague for a consultation, in accordance with a relatively formal structure for consultation resources and fees. A consultation is paid for and it was perceived by the respondent physicians as quite a different matter from a conversation with a colleague. The physicians identified in response to the sociometric questions tended to be medical school classmates of the respondent and thus not likely to be perceived as being especially more knowledgeable than the respondent. The other major group identified was that of administrative superiors in institutional contexts.

The respondents would tend not to call on an older and more experienced physician because, as one respondent put it, "experience is a nebulous thing. In thirty years of practice, a man may have seen only one case of the type that I may be interested in." Another noted that, "I used to discuss my cases all the time when I was at the hospital regularly. In a hospital where there is a teaching setup, everything is geared for discussion and interesting cases are always discussed. But that has all changed since I have been in full time private practice. Now, I don't go out of my way to mention something to another doctor just because it's an interesting fact. Naturally, if it were a primary problem really important for a patient's welfare, I would call a specialist for a consultation." Another noted that "there are five doctors on this block and I barely know them. We're all too busy."

There were some physicians who seemed especially likely to feel free to telephone other physicians to discuss a case. The general practitioners who regularly referred patients to specialists seemed to feel that they could telephone the specialists to discuss a case. Physicians at hospitals, medical groups, clinics, or in any other situation where they saw other physicians, might discuss their cases with others, but this was likely to be on a relatively adventitious basis with whomever was available while waiting for a meeting or the like, rather than regularly with the same few persons.

It was speculated that there might be some correlation between the physician's medical specialty and his comparative contact with others, as measured by the three sociometric questions. The comparative ranking of the several specialty groups studied, in terms of the mean number of mentions of physicians in response to the three questions, showed internists and psychiatrists-neurologists in first and second place. Considerably behind these two fields were general practitioners, obstetricians-gynecologists, surgeons, pediatricians, and ophthalmologists and eye-ear-nose-throat specialists. The fields which reflect the most social interaction, psychiatry-neurology and internal medi-

TABLE 6

Average Rank of Sources of Reliable Information on New Drugs, in Terms of When Physicians Prescribed Chemneo

	First Prescription within 2 Months After Introduction (N=18)	First Prescription within 2-4 Months After Introduction (N=49)	First Prescription within 4-6 Months After Introduction (N=116)	First Prescription within 6-8 Months After Introduction (N=149)	First Prescription within 8-10 Months After Introduction (N=101)	Non- Adoptors (N=1,013)
Medical Journals	1.3	1.3	1.1	1.2	1.4	1.3
Detail Men	.9	1.0	.8	.7	.7	.8
Direct Mail	.3	.2	.3	.1	.3	.3
Medical Meetings	.1	.1	—	—	.1	—
Research Center	.1	.1	—	.1	—	—
Hospital	.1	.1	.1	—	.2	.1
Colleagues	.1	.1	.1	.2	—	—
Samples	.1	.1	.2	.2	—	.1
Druggists	—	—	.2	.2	.1	.1
Lay Mass Media	—	—	.1	.1	—	.1
Patients	—	—	—	.1	.1	.1
Physician's Desk References	—	—	.1	.1	.1	.1

cine, are both very broad-based fields of which the boundaries have been continually expanding. It is thus possible that the practitioners of these two specialties may have more substantive occasion to discuss their cases with others.

How did the physicians who prescribed the drug hear about it? It was felt that it would not be possible to obtain a realistic answer to this question by asking the interviewees to attempt to recreate their thought processes. It was thought, however, that a realistic answer might be obtained to the question: "What three sources, in the order of their importance, would be most likely to provide reliable information on a drug like Chemneo?" It was decided to analyze these data in terms of a comparison of the sources that were most important to the total population. The population of physicians who had prescribed the drug, composed of both the original sample as well as the interacting physicians, was divided into five groups based on when the drug had first been prescribed. A source which was named first was given a score of 3, one which was second was scored 2, and one which was named third was given a score of 1. The maximum score possible was thus 3. The results are shown in Table 6.

These results are more suggestive than definitive because of the small number of respondents in some of the key early-prescription groups, and because

of the extent to which some respondents may have given answers which they believed to be socially acceptable. There appear to be only minor differences between the innovator group which tried the drug in the first two months and the group which tried it in the next two months. The physicians who were slow in trying Chemneo differ from the more rapid adopters in their tendency to pay some heed to non-professional sources, while the more rapid adopters seem to be more sensitized to professional sources. For all groups, however, the traditional commercial sources of journals and detail men seem to be the most important sources of information about a new drug. Colleagues rank relatively low with all groups. As one respondent said, "I naturally have more faith in a double blind study that I have read than in something that someone tells me." The importance of the nature of the drug whose diffusion is being studied is suggested by one respondent: "It all depends on the drug. If it is a real innovation like a new steroid or anticoagulant, it will be discussed by everyone." It can be speculated that detail men are perhaps more popular sources of information in a smaller community, because they provide breaks in the physician's routine. In a big city, the physician probably is less likely to welcome such breaks.

CONCLUSION

This study is not a pure replication of the earlier study, because of the several differences noted above. Therefore, the phenomenon being measured and reported may *not* be the same as that reported in the earlier study. Chemneo may be atypical of other drugs, or it may be typical only of one family of drugs or only of drugs which achieve its degree of acceptance. If the phenomena reported in the earlier study and in the current study could be shown to be analogous, it would be possible to speculate that the process of communication among physicians in a large city differs from that reported in a smaller community. In such social communication as does occur in a large city, it is possible that the special attributes of a new drug may not figure meaningfully as a conversation topic among sociometric partners. Physicians hear of a new drug like Chemneo and decide to try it and adopt it on the basis of various factors, but sociometric choice does not appear to be significantly correlated with use of the drug and thus does not appear to be among the significant factors. The professional practices of physicians in a large urban medical center and in a small community thus differ at least on this dimension, insofar as our findings on Chemneo are generalizable.

It is possible that the physicians in smaller communities are in a *Gemeinschaft* kind of situation in contrast to those in a big city who tend to be in a *Gesellschaft* situation. The theory of innovation which accurately reflects the process of innovation and diffusion in a smaller community may not do so

in at least one large urban medical center. It is possible that the community studied is atypical. It is also possible that the relationship of physicians to their community is so unique that it is not possible to generalize about the behavior of physicians in large cities. These findings suggest the possibility, however, that the large-city pattern of diffusion of innovation of a new drug may be dependent on more impersonal methods of communication than is the case in a smaller community.

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The Social Dimensions of a Twelve-Man Jury Table¹

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The term "face-to-face," frequently used to delimit the interest of small-group research, has not usually been interpreted literally. If there are more than two persons in a group, it is not possible to seat them so that they are fully face-to-face, and if there are more than three persons, it is not possible to seat the group so that each member has equal visual accessibility to the remaining members. So long as the groups are relatively small the differential accessibility between positions is usually small, but it increases rapidly for groups larger than six seated at rectangular tables. For the conscientious hostess, the recognition that all positions are not equally accessible raises delicate social questions. Social rank must be recognized and, beyond this, the consequences to be promoted or avoided by accessibility or isolation must also be considered. For the student of small-group behavior, the differential accessibility between positions may have implications for research design, but even if this is not so, better knowledge of concomitants of position may increase the understanding of mechanisms involved in interpersonal communication.

Four recent articles dealing with the importance of relative seating in discussion groups deserve specific mention in connection with the present report. Steinzor (12), working with a counseling class seated in a circle, reported that, after any given person stopped talking, persons directly opposite (and consequently, most distant) were most likely to speak next. From the position opposite the speaker to the position adjacent to the speaker, the "speak-next" probability declined. Steinzor concluded that the more nearly opposite one sits, the greater the physical and expressive stimulus value of a speaker. Unfortunately, Steinzor's method of analysis does not rule out the possibility that his results may have arisen simply because two high participators in his group tended to sit opposite one another.

Bass and Klubeck (1), who were interested in relative participation as an indicator of leadership in groups seated in a V-formation, realized, as a result of Steinzor's study, that participation (or leadership) might involve a positional bias. They extrapolated from Steinzor's result the hypothesis that the person with the "greatest mean seating distance from all the other partici-

¹ The authors wish to acknowledge R. P. Abelson's helpful criticism of the version of this paper presented at the Midwestern Sociological Meeting in April, 1956 and for earlier discussions in 1951. More recently, Jack Sawyer, Anthony Kallet, and Robert Sommer have made helpful suggestions. This work is part of the Jury Project of the Law School, the University of Chicago, and this draft was completed while Fred L. Strodtbeck was a Fellow at the Center for Advanced Study in the Behavioral Sciences.

pants would be likely to attain higher leadership status." However, they also noted that Harris had stated: "To arrange (the participants) in a semi-circle—the chord of which is occupied by observers—is psychologically bad. The end candidates of the semi-circle are isolated, the central candidate is spot-lighted." When Bass and Klubeck checked their data for 61 groups, they concluded that neither the extrapolation from Steinzor (the ends would predominate) nor that from Harris (the ends would be isolated) was confirmed in their composite results.

In the light of our more recent experience, we believe that Bass and Klubeck erred in trying to group their various patterns together and, if we average their measure of leadership for each of the different size groups for parallel positions, the data suggest some regularities.

Six-man groups

(3)	40	40	(4)	
(2)	38		38	(5)
(1)	47		47	(6)

Eight-man groups

(4)	35	35	(5)	
(3)	41		41	(6)
(2)	39		39	(7)
(1)	42		42	(8)

It may be noted that the advantage of the first (and sixth) men in the six-man group is shared by the first and third (and sixth and eighth) men in the eight-man groups. The pattern is disrupted in the seven-man group where the presence of a man at the point of the V clearly shifts participation away from the end-man:

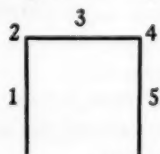
Seven-man groups

	(4)			
	40			
(3)	40	40	(5)	
(2)	38		38	(6)
(1)	35		35	(7)

Although these authors did not interpret their data in this way, one guesses that participation is more determined by visual accessibility than by distance. In the circle these factors are simultaneously present in the person opposite; in the seven-man formation it is the persons with central locations and shorter mean distances who participate most, possibly because the presence of a person at the point of the V shifts visual scanning away from the ends.

Further information about these matters has become available from recent papers by Sommer. In his first paper (10), he reports observations using primarily an eight-person 1-3-1-3 position, rectangular table. Naturalistic observation in a lunchroom suggested that persons talking with one another were most frequently seated at right angles, in an end-corner relationship to one another; next most frequently side by side; and least frequently, face to face across the table. To determine whether people expecting to talk with one another seek the right-angle relationship, he asked normal subjects to take seats and discuss a proverb. He found that the end-corner relationship was selected about 80 per cent of the time. With identical instructions, schizophrenics picked this relationship less than 15 per cent of the time. By using a decoy already seated at the corner position of the table, he indirectly confirmed that the end-corner was the "closest" position psychologically, by showing that it was more often chosen by like-sex decoy-subject pairs than by cross-sex pairs; more by women than by men; and least of all by schizophrenics. Finally, by placing his decoy in the middle of the three place side, he demonstrated that directly opposite was the favored cross-sex location, but that women preferred to sit alongside women decoys. Sommer (11) also has demonstrated that, in groups of four, five, and six, leaders prefer to sit at end positions; other members then prefer to sit corner to the leader. In those groups where the leader sits in the corner chair, other members choose to sit either in the adjacent end position or in the opposite corner. These findings indicate that being visually accessible to a leader is an important consideration in the selection of seats by subjects.

In an unpublished memorandum from the Bales laboratory at Harvard, Bernard P. Cohen reports in passing that end positions for a five-man table, as illustrated below, do not produce higher participation. But, more



importantly, the Cohen data show that members speak more frequently to adjacent members during "social" (in contrast with "task") portions of the group interaction. Taken together, these fragmentary findings suggest that, if there were available a more standardized way of associating social effects with circular, V-shaped, rectangular and other spatial arrangements, then one might find a way to predict full-face as against 90-degree preference and possibly predict social effects of arrangements which had not already been studied. As a step toward this objective, a simple distance analysis of social effects attributable to table position in jury deliberations

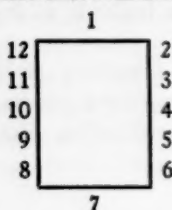
will be presented to illustrate what one may infer from visual inspection of the data. Then multidimensional scaling will be used to construct a Euclidean model as an aid in the identification of important dimensions.

SOURCE OF DATA

Data from 69 experimental jury deliberations carried out under standardized conditions are to be utilized in the analysis. The participants are jurors drawn by lot from the Chicago and St. Louis courts, who listen to a recorded trial, deliberate, and return their verdict—all under the customary discipline of bailiffs of the court. The jurors are called by lot from a pool of about three hundred persons; they walk accompanied by a bailiff to the deliberation room, and, while they enter the room at the same time, those at the front of the queue have a wide choice of seats around the 1-5-1-5 position rectangular table, whereas the last few have little choice but to fill in the vacancies. In sex and socioeconomic status, the jurors are quite representative of the registered voters in the two communities where these data were collected.

The jurors' first task is to elect a foreman. By study of composite tabulations rather than the deliberations, one may note that the selection of the foreman, which proceeds with such deceptive casualness, is by no means a random process. Considering position alone, it is possible to classify the 12 positions around the table into four types.

Reference Numbers

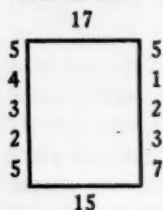


Position Types

End	1, 7
Corner	2, 6, 8, 12
Flank	3, 5, 9, 11
Middle	4, 10

In terms of these types, the foreman is most frequently selected from one of the two persons seated at the ends of the table. The distribution of foreman by position was as follows (14):

Distribution



Average

End	-16.0
Corner	-5.5
Flank	-2.5
Middle	-2.5

This is clear indication that the jurors felt that there was some intrinsic "propriety" about the foreman being at the head of the table. Sommer's second study, in which he shows that leaders have a preference for the end positions, is congruent with this finding. It is even possible that a move to select someone other than a person at the end of the table would be perceived as a rejection of the persons on the end, while the selection of a person at the end would not offend anyone because it could be presumed that being at the end was accidental.

There is just a suggestion in the data that the selection of a seat at the time of entry into the room was not random. Proprietor-and-Manager class persons took seats in the End position about 15 per cent more frequently than would be expected under a random distribution and in the Corner position 18 per cent less frequently than would be expected if seating were random. The remaining index values were close to unity. To the extent that positions were available for selection once the juror had arrived at the deliberation room, this technique parallels Sommer's. But, in contrast with Sommer's finding of marked position effects (when two persons choose two places at an eight-man table), position effects by socioeconomic class (when twelve persons seat themselves at a twelve-man table) are small.

This is not imply, however, that occupation is unrelated to being chosen a foreman, for, although Proprietors were only slightly overrepresented in the end positions, they were strongly overrepresented among those chosen as foreman. The index values relating to frequency of choice by occupation are as follows: Proprietor, 1.95; Clerical, 0.81; Skilled, 0.92; and Labor, 0.63. Thus, while the jury frequently gave as its rationale the explanation, "You take it, you are at the end of the table," the evidence strongly suggests that they had scanned both ends of the table before making this decision.

The role requirement of being foreman increases relative participation of the incumbent until he accounts for approximately 25 per cent of the total interaction process acts.² It is therefore not surprising to find that the End position is significantly differentiated from the other three in terms of the average percentage of acts originated by position (based on 29 juries):

Position	Per Cent of Total Acts
End	10.0
Corner	7.5
Flank	7.8
Middle	8.4

² No correction has been made in this analysis and those following for the differential presence of the foreman, for it was believed that the position effects implied in his selection should be permitted to continue to be present in measures based upon his subsequent functioning.

Since there are twelve positions, the baseline expected per cent of total acts is $8\frac{1}{3}$. It may be noted in passing that the Middle position, which had been lower than the Corner and the same as the Flank on distribution of foreman, is somewhat higher than the other two on participation.

If it were possible to obtain thoroughly reliable matrices of who speaks to whom, particularly if there were a way to distinguish "one individual speaking to another" from a speech to the "group as a whole," then one might carry out an analysis of position effects based directly upon interaction protocols. Under the communication situation which prevails in the jury, this is not indicated because messages between individuals in a group of twelve are heard by most of the other participants. What is said is very important though; the correlation, within a jury, of relative participation and post-meeting sociometric votes is approximately 0.69. The sociometric votes in this correlation, and in the present analysis, were obtained after the deliberation by having each juror check four sociometric choices on a diagram of juror seat positions. Half of the jurors in each jury responded to the question: "Which four of your fellow-jurors really helped the group arrive at its decision?" and the remaining half responded to: "Which four of your fellow-jurors would you best like to have serving if you were on trial?" Since there were no significant differences in the ranking within individual juries by these two criteria, the results were combined to produce the following distribution of votes received by position:

Position	Votes Per Position
End	300
Corner	241
Flank	248
Middle	274

It may be noted that the gradient by percentage of total acts per position is essentially re-created by the votes received. The End position is again highest, the Middle position is second, and the remaining two are similar to each other and lower than the other two. However, this time we are more fortunate than we were in the case of the interaction process acts, for we can distribute the sociometric votes by originator and target into the symmetric matrix given in Table 1.

Included with each entry in Table 1 is an alphabetic designation for the 21 unique patterns of relationships between positions. For example, all four End-to-Corner positions (1-2, 1-12, 7-6, 7-8) are given the same designation *a*. Geometrically this is the equivalent of taking the first 1-2 relationship, then revolving it about the End axis and again about the Middle axis. One might tentatively designate as *a'* the corresponding set of relationships which

would arise from an interchange of target and originator in the four *a* relations; namely, 2-1, 12-1, 6-7, and 8-7. In Table 1 no distinction is made between the *a* and *a'* type of relations because votes interchanged are to be used as an *inverse* measure of the distance between the target and originator, and it is desirable that the distance between the target and originator be the same as from originator to target, i.e., no greater in one direction than in

TABLE 1
Between-Position Patterns and Votes Exchanged

Originator Positions	Target Positions											
	1	2	3	4	5	6	7	8	9	10	11	12
1	—	23a	24b	22c	24d	21e	25f	25e	23d	25c	23b	24a
2	33a	—	18l	26j	18r	19u	22e	26t	20o	27k	23n	27s
3	32b	21l	—	32h	24q	20r	20d	21o	19p	28i	26m	15n
4	32c	21j	24h	—	24h	25j	25c	21k	23i	30g	20i	14k
5	28d	18r	18q	25h	—	21l	33b	27n	28m	26i	20p	15o
6	31e	22u	19r	25j	24l	—	25a	27s	19n	25k	24o	18t
7	26f	26e	18d	24c	24b	24a	—	24a	26b	22c	31d	14e
8	25e	26t	24o	30k	30n	27s	26a	—	23l	16j	17r	15u
9	29d	17o	22p	31i	27m	26n	23b	25l	—	26h	20q	13r
10	32c	25k	21i	21g	21l	22k	24c	24j	24h	—	26h	19j
11	26b	27n	26m	22i	21p	16o	29d	23r	17q	26h	—	26l
12	31a	28s	21n	21k	21o	17t	24e	30u	18r	22j	26l	—

the other. In addition, we wish to smooth out minor perturbations. To accomplish both of these objectives, we average the eight *a* relations and obtain the value 26.25. Thus, since twelve cells of the main diagonal are empty, all required values for the 132 cells of the smoothed matrix can be given in a compact tabulation of the average votes interchanged over the 21 unique patterns in Table 1.

a 26.25	h 25.88	o 19.75
b 26.36	i 24.00	p 20.50
c 25.75	j 22.25	q 19.75
d 25.25	k 23.12	r 18.25
e 23.50	l 23.00	s 27.25
f 25.50	m 26.75	t 21.75
g 25.50	n 23.50	u 21.50

SIMPLE DISTANCE RELATIONSHIPS

The use of the smoothed values above can be illustrated by testing first the proposition that the votes exchanged between the End position and other positions along the side of the table will be so ordered that the shorter the physical distance the greater the number of votes. By reference to Hypothesis I, Figure 1, one may see the inequality involved and the patterns in question. When the smoothed values are substituted, one transposition of *a* and *b* is required to match the prediction based upon distance alone. More detailed investigation of the values is particularly interesting, for it reveals that the hypothesis is regularly supported by the Side-to-End votes, but not by the End-to-Side votes.

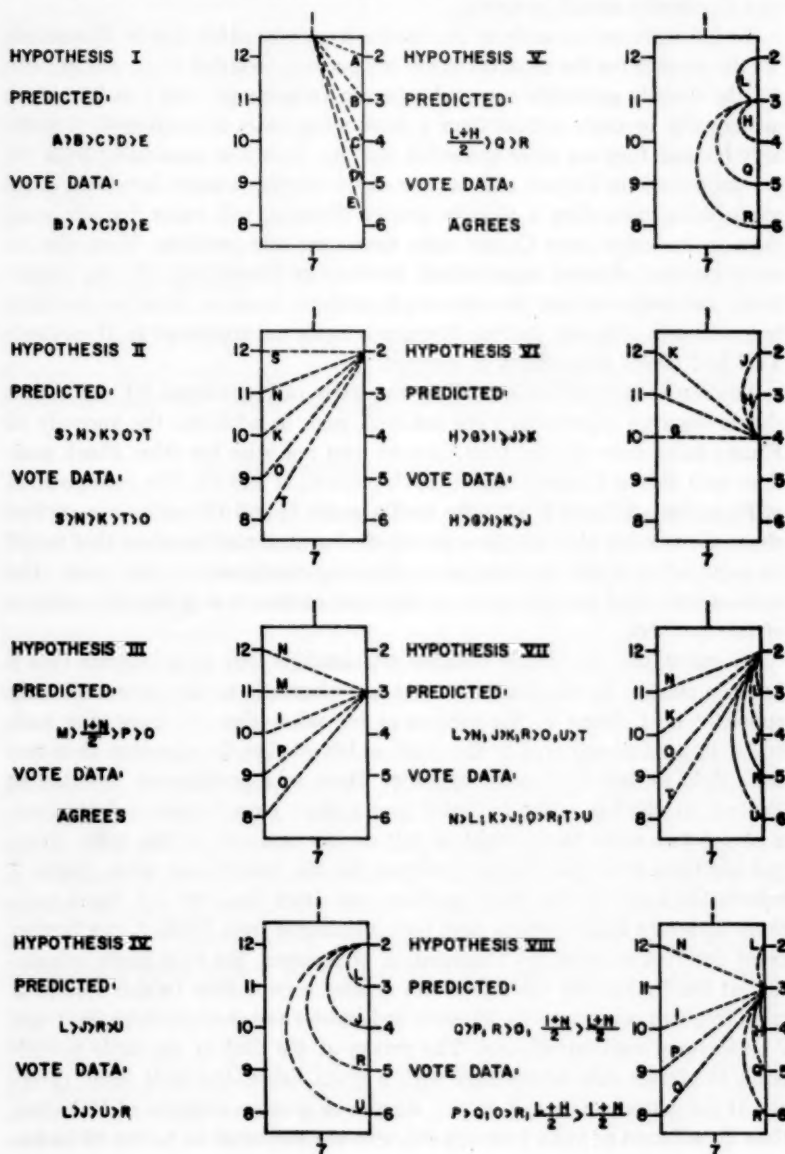
	Side-to-End	End-to-Side	Combined
a	28.75	23.75	26.25
b	28.50	24.25	26.38
c	28.25	23.25	25.75
d	26.50	24.00	25.25
e	25.50	21.50	23.50

This reminds one that both End and non-End persons had the same limitation of 4 votes in this application. Thus, the unusual social propinquity which Sommer's work indicates is given by right angle orientation may have operated to make Ends "closer" to more persons than subjects on the sides were. If this were true, End persons would be under greater "demand" for their votes, and thus the "fixed number of votes" procedure may introduce a non-symmetric character to the measure of social distance. One should not go too far with this interpretation, for this first test has involved only five of the 21 relationships. The remaining 16 patterns provide many further independent checks on variations in votes interchanged by physical distance and angle of orientation. Seven of these additional checks are given in Figure 1.

The votes interchanged between Corner positions and positions on the opposite side of the table (see Hypothesis II) indicate a marked Corner-to-diagonal-Corner preference and a tendency to slip over the distant Flank position. The more detailed breakdowns suggest that the lower number of votes arises not so much from "skipping" by the Corner originator as from a disposition by the distant Flank position not to reciprocate in sending votes back to the Corner. This finding of the Corner being closer than the Flank in the cross-table instance is replicated in the same-side tests given under Hypothesis IV. In this case there is no sending-receiving imbalance; the votes are simply exchanged as if the same-side Flank were more distant than the same-side Corner. These two findings taken together indicate that, if a

FIGURE 1

Test, by Votes Interchanged, of Eight Inverse Physical Distance Hypotheses



"point-to-point" distance representation were sought which would correct for differences in angle between positions, then, in all probability, more than two dimensions would be needed.

To illustrate one anomaly in the results, it may be noted that in Hypothesis VI the average for the adjacent same-side persons (relation h) is greater than for the visually accessible cross-table person (relation g). But i and k, which are equally or more distant than j, have more votes interchanged, presumably because they are more accessible visually. However, continuing from the perspective of the Corner, one can test again whether a more favorable visual angle, notwithstanding a slightly greater distance, will cause far-side positions to exchange more Corner votes than near-side positions. Here, the inverse physical distance expectations involved in Hypothesis VII are consistently not confirmed and the alternate hypothesis based on visual accessibility is consistently affirmed. Inverse distance is again not confirmed in Hypothesis VIII and visual accessibility is affirmed.

The reader may satisfy himself by the study of Hypotheses III and V that simple distance expectations are satisfied, and, in addition, the anomaly of Flanks being more distant than Corners does not arise for other Flank positions as it did for Corner positions in Hypothesis II and IV. The juxtaposition of Hypothesis III and V with the results under II and IV makes one curious about the number of dimensions and kind of spatial configuration that would be required to make the interpoint distances consistent in one space. The multi-dimensional analysis given in the next section is a systematic solution of this problem.

To summarize the simple distance relationships, our data indicate that a juror is closest, in the sense of votes interchanged, to the person directly opposite; next closest to the persons at his sides; then, in decreasing rank order, to persons one seat to the right or left of directly opposite, then two seats right or left of directly opposite. These relationships are reported in Table 2, in which it is also indicated that a given juror is quite isolated from a person two seats to the right or left on his own side of the table. Since ± 4 positions represent Corner positions for the twelve-man table, Table 2 reflects the anomaly that these positions are closer than the ± 3 Flank positions. Although End positions have been eliminated from Table 2, one further point deserves mention. In confirmation of Sommer, the right-angle orientation at the End of the table produces greater accessibility (which results in more frequent occupancy by foreman and greater required participation) and does increase vote interchange. The person at the End of the table participates in greater vote interchange with a given side-of-the-table juror (average 51 for patterns a, b, c, d and e), despite an average distance of 52 inches, than do adjacent persons (average 49) who are estimated to be but 19 inches

TABLE 2
*Distance and Votes Exchanged at a Twelve-Man Table**

Relation to Juror	Pattern	Distance in Inches	Average Votes
Opposite	s, m, g	32	53
Own Side ± 1	l, h	19	49
Opposite ± 1	n, i	37	48
Opposite ± 2	k, p	50	44
Opposite ± 4	t	83	44
Own Side ± 4	u	76	43
Own Side ± 2	q, j	38	42
Opposite ± 3	o	66	40
Own Side ± 3	r	57	37

* End positions excluded.

from one another. When the End is excluded, the case for simple distance is stronger, but not unequivocal. The correlation between distance and the smoothed values for votes interchanged in Table 2 is $r = -0.58$.

MULTIDIMENSIONAL RECONSTRUCTION

If it is granted from Table 2 above that votes exchanged and distance (in inches between head positions) do not correlate highly, then one is curious to know what shape a constellation of points would take if each point representing a position at the table were separated from each other point by a distance inversely proportional³ to the square root of the values in the smoothed "votes exchanged" matrix. Or, to state the objective more technically, one seeks the smallest number of dimensions which, with some allowance for error, will fit the points into a real Euclidean space.

The earlier work which guides the present application is a generalized solution suggested by Messick and Abelson (6) which, with some shift in notation, is available with an example in Torgerson (16). Unfortunately, the matrix notation employed may prove troublesome for many who might otherwise wish to study the computing routine. In addition, the requirement of at least three principal-component factor analyses as a part of the iterative process used to determine the required constant will probably discourage persons dependent upon a desk calculator—particularly if their application

³ It may be observed in passing that, while the function of "inverse distance" one might choose to use is unrestricted, it is not clear how the resulting configuration would be changed by different choices. Even the social physicists, with their great admiration for "inversely proportional to the distance squared," would at times let the exponent of distance be a variable, i.e., Stewart's index of penetration (13). Similar investigation is probably warranted in this area of application also.

involves as many as twelve points. In the present case, so that the computations will be available to others, a supplement has been filed with the American Documentation Institute which, using Torgerson's notation, gives the details of the following steps: ⁴ (a) adjust the set of relative interpoint distances so that the smallest value is zero to produce h_{jk} ; (b) compute required constants by operations on h_{jk} ; (c) select c , the constant, and form coefficients to obtain the sum of the diagonal elements of the B^* matrix; (d) obtain the B^* matrix of the inner products of the vectors formed by joining the centroid of the points to each of the points; (e) extract the principal components of B^* (only the results are shown, as it is assumed that the steps may be looked up in reference 15); (f) check the assumed value of c to determine a new value, and substitute in step (c) above; and finally, since one iteration sufficed, (g) plot the result of the principal component analysis from the matrix built with a satisfactorily correct value of the additive constant, c .

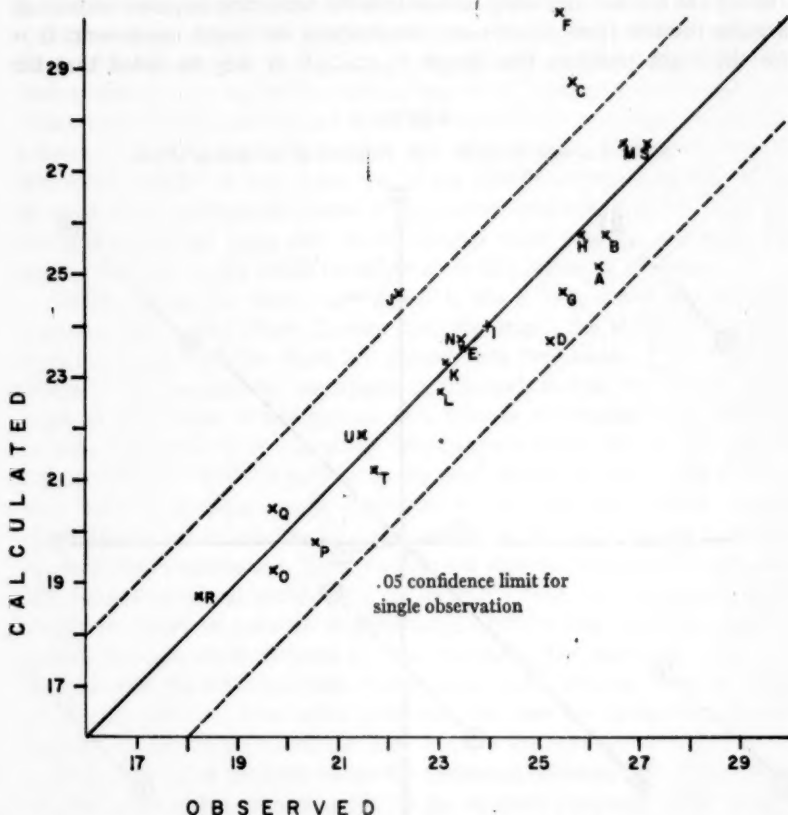
The primary result of the calculation is the following locations of the positions on the three principal dimensions:

Position	I	II	III
End 1	.484	— .122	.018
7	— .484	.122	— .018
Corner 2	2.078	1.890	— .894
6	—2.072	1.905	— .937
8	—2.078	1.890	.894
12	2.072	1.905	.937
Flank 3	2.426	—1.408	.903
5	—2.465	—1.359	.925
9	—2.426	—1.408	— .903
11	2.465	—1.359	— .925
Middle 4	0.000	—0.911	1.141
10	0.000	—0.911	—1.141

The distance between each of the unique patterns of points can be obtained using standard algebraic formulae and (after appropriate addition of constants, determination of reciprocal and squaring) the new, fitted values can be compared with the original values in the scattergram shown in Figure 2. The fit is quite good; only pattern *f*, the pattern linking End position 1 and End position 7, is seriously in error. In this case, the calculated votes exchanged is 30.2 and the observed is 25.5. Eighteen of 21 values are within the .05 confidence limit.

⁴These computations have been deposited as Document number 6907 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. To obtain a copy remit check or money order for \$3.75 for photoprints or \$2.00 for 35mm. microfilm, payable to the Chief, Photo Duplication Service, Library of Congress. Cite Document number in ordering.

FIGURE 2
Observed and Calculated Votes Exchanged *



* See Table 1 for a key to the between positions patterns indicated by the alphabetic letters.

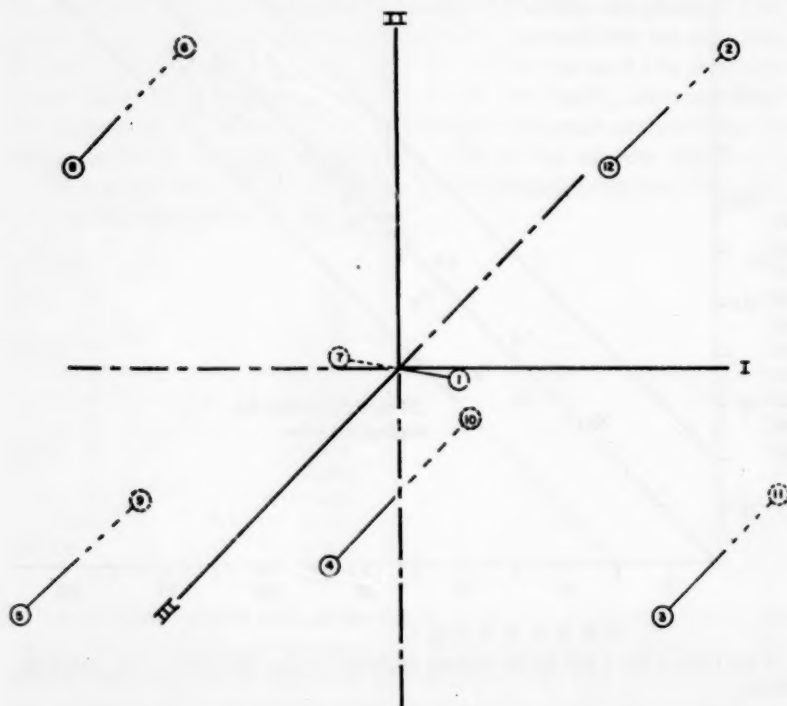
In Figure 3, the factor values are plotted with Factors I and II used for the conventional x and y axes, and Factor III is plotted with a 45-degree projection. The reader should permit the solid extremes to rise vertically up on the page and the dashed extremes to sink below.

This figure may first be inspected to determine the degree to which it "explains" the regularities set forth in the previous section. First it may be noted that the Middle positions, 4 and 10, have a zero location on I. If Factor I is associated with the length of the table, then their position is such

that no contribution to their social distance is made by the length component. For the End positions, 1 and 7, it may be inferred that their favored position (facing one another and being accessible to the remaining members with small angular rotation from direct view) foreshortens the length component. It is for the Flank positions that length is greatest. It may be noted that the

FIGURE 3

Social Distance Between Seat Positions at a 12-Man Table



plotting for both 5 and 9, and 3 and 11 on Factor I is more distant from the origin than is the plotting of the Corner positions, 6 and 8, and 2 and 12. This line of interpretation requires one to think of negative values simply as distance to the left of the origin, not as a low loading on Factor I. The fact that the Corners have an intermediate location between End and Flank on Factor I might be explained in at least two ways. First, there may be a sort of serial learning effect such that a stimulus at the end of a series becomes familiar more quickly. But, much more probably, the persons who

sit at the Corners can lean further forward, perhaps even "scooch" around the Corner a bit, so that they enjoy the favored orientation of the Ends to a degree that is denied Flank members.

Following this same line of interpretation (leaving aside for the moment Factor II), Factor III may be associated with the width of the table. The End positions have negligible contributions from width and the Corner and Flank and Middle positions are separated by slightly increasing width components. This differing width may in part be an artifact of the limitation to the stated number of four votes, for, if the Middle person is socially closer to more other positions by virtue of his central position, then his votes exchanged across the table may be distributed more broadly, resulting in a greater estimate of the width contribution in this region of the table.

Finally, Factor II, which corresponds to the y axis, orders the positions with the Ends a very slight distance from the origin, the Middle less distant than the Flank, and the Flank less distant than the Corner. This dimension brings to mind a point left ambiguous in Sommer's finding that the preferred angle of orientation of two persons at a table is 90 degrees at an adjacent Corner. For, while such a position permits great proximity, it also permits a participant to "look his partner in the eye" with a mutual 45 degree rotation, and it is perhaps equally important to note that this position permits either participant to avoid looking his partner "in the eyes" by simply returning to a direct orientation. Thus, one cannot separate proximity from mutually controlled, visual accessibility in Sommer's data. In the present application, it should be possible to disentangle distance and visual accessibility contributions to social distance as they operate in the contrasts of the End position with the other positions. For example, End positions have the greatest average distance from other positions, but they are uniquely accessible visually. Of all the positions, the Ends require the minimum rotation of the head or inclining of the body to see the remaining positions. The Corner position has a somewhat extreme angle for the opposite End and would actually have to lean forward to see his same-side Middle, distant Flank and distant Corner positions. For the Flank position, this angular positional disadvantage is less extreme, for an occupant has only to lean forward to see two other participants, his same-side distant Flank and distant Corner position. For the Middle, there are also two hidden positions, the same-side Corners, but the data suggest that this does not result in as great isolation as is true for Flanks, perhaps again because the Corners "scooch" around toward the End. The analysis suggests that a construct, "visual accessibility" or, perhaps, "angular advantage," while not as conventional as length and width, would account for the contributions to social distance arising from Factor II.

This analysis of the components of social distance in a face-to-face group

into length, width, and visual accessibility enables one to return with new tools to the conceptualization of the advantages of the End (and Middle) positions in contrast with the disadvantages of the Corner (and Flank) positions. It appears that the low isolation of the End positions arises from negligible weightings on width and accessibility, and only a moderate weighting on length. The Middle position has no weighting on length, a moderate weighting on accessibility, and a higher weighting on width. While the remaining positions are not differentiated on width, they are markedly weighted on length and accessibility. In terms of their relative position in the configuration, the juxtaposition of Corner and Flank indicates that in the diagonal from a Corner to a same-side distant Flank is the maximum social distance, and thus represents validly the apparent anomaly of "Corner to distant-Corner distance" being less than "Corner to distant-Flank distance," both for same-side and cross-table relations. The net effect is to place the twelve points as a configuration with the End position in the center and the remaining positions in a W-shaped band with the Corner positions at the tips of the W and the Middle positions at the point as shown in Figure 3.

DISCUSSION

The method used for determination of the configuration is completely general. Whenever there is an interposition matrix which constitutes a reasonable index of social interbehavior, a comparable determination of points and the relation of points to spatial characteristics may be carried out. The social-psychological processes which give significance to position will vary as one moves from face-to-face groups to work positions in a company or dwelling locations in a community, but, underlying the social process in a wide variety of locations, it is believed that one will find some variant of Homan's proposition (4) that "increased interaction results in increased liking."

The institutional context of the court in which the jurors serve lends importance to their experimental task. Unlike one's preference for lemon or cream in tea, the issues of the deliberation are highly salient in Newcomb's (7) sense of the term. The jurors sincerely wish to reach a genuine consensus of attitude. More than that, the jurors enter their deliberation with a real apprehension that they may be coerced into accepting a verdict with which they do not agree. As Schachter's (8) work has recently demonstrated, these apprehensive jurors are in the psychological frame of mind in which one tends to look for a friend, particularly while they are getting adjusted to the new situation. Mills (5) has shown that a positive interpersonal feeling generated at the beginning of a discussion is slow to be relinquished, even if marked

differences later arise. Jury materials show that no fewer "votes" are given to jurors who agree with the respondent on the verdict than jurors who "disagree," once a correction for the amount of participation has been made. Therefore, while similarity of belief is less important than one might have supposed, it is still randomized between positions. While the amount of participation is important, in the present instance all differences, save those which exist between the four classes of positions, are smoothed out by the averaging of the results for the like positions of the 69 juries. The effect that remains is a reflection of the fact that certain interpersonal responses have been made more probable and others less probable by a juror's location in a given position—this is the "spatial" consideration at issue.

These spatial effects come into operation after selection of a chair when the juror settles into his chair. After having been called by lot and marched in by the bailiff, the juror probably feels a need for something which he can recognize as *his* place. And, since there are numbers by the positions, his position also becomes his identification and the vantage point from which he learns his interpersonal environment. As the conversation shifts back and forth around the table, the juror can usually see the speaker, and, in addition, in a half-conscious way, he can see the reactions of those most visually accessible to him. The reactions he scans include the accessible jurors' reactions to the experimenter, to the trial, to the comments of others, and most particularly, reactions to the reference juror's own contributions. As this full range of reactions is watched, the reference juror will note, in the majority of instances, that the accessible juror responded in a reasonable and consistent way. No mysterious mechanism is believed to be involved, for, social animal that man is, he likes better people that he comes to know than those he does not, and he is highly motivated to come to know people when he is apprehensive at the outset of a group task requiring consensus.

There are undoubtedly individual and cultural differences in the response to the experience which a position around a twelve-man table represents. A Navaho, who thinks it is rude to look in another person's eyes, could conceivably perform differently from a randomly selected urban American. But would it not be preferable to advocate skepticism concerning unique cultural patterns until they have been carefully documented? At present, persuasive arguments for cultural uniqueness, like those of Hall (2) in his discussion of "Space Speaks," cannot be articulated with workaday social psychology because too little of the required parametric observation is available. If, only to oppose the cultural uniqueness position, one were to formulate a *comparative* social psychological position, then students might be motivated to analyze cultural premises underlying space usage in terms of threat, affiliative needs,

status differences, etc. under legitimation of a research orientation which holds that the mechanisms for setting spatial boundaries are constant in all cultures.

In the same spirit of appreciation and caution, it may be observed that one cannot work in this area without acknowledging the great debt to the animal ethologists—particularly for their highly ingenious methods. It is fun to know from Hediger (3) that the flight distance for lizards is 12 feet and, for crocodiles, 50. But, despite our appreciation of these new and fascinating findings, it is equally valuable to re-emphasize Simmel's concerns with spatial boundaries as ever-shifting determinants of interaction (9, pp. 467-470) and with space as a sociological category (9, pp. 460-526) which is restructured as functional reciprocity develops. One must be cautious not to let position, emphasizing space and distance as it does, be swept up entirely in the gram-centimeter-second traditions of physical science. Within the tradition of small-group research, there is a prospect for the disciplined social investigation of position, hopefully without reducing it to physics or phylogeny or letting it become lost in yeasty cauldrons of "communication" or "culture." The translation of interpositional matrices into point spaces by the techniques illustrated here will generally reduce the number of dimensions and should thereby facilitate parametric comparisons. By such steps, a properly subordinated study of position effects can be worked into both the technology of small-group research and the social psychology of what is now frequently described as bio-communication.

SUMMARY

The multidimensional scaling (using a distance model) of a matrix of post-session preferences of jurors resulted in the identification of table length, visual accessibility, and table width as the three primary components of "social distance." In this application, "social distance" was postulated to be inversely related to the square root of preferences received. The three derived dimensions were, in turn, used to characterize the differential attributes of positions with the result that the End and Middle positions were shown to have smaller contributions from each of the components of "social distance" than the Flank and Corner positions. The "nearness" of a person opposite a given juror (or at the end of the table) is believed to have arisen because his reactions to the trial could be inconspicuously watched, and, when this was done, the reference juror tended to deduce that he was like-minded and dependable to a greater degree than jurors whose reactions were less easily observed.

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The Effects of Continued Practice on the Behaviors of Problem-Solving Groups¹

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AIM

This study was intended to find out whether the performances of groups working continuously in highly centralized and in decentralized communication networks for relatively long time periods would differ from those reported in studies of shorter duration. Bavelas (1,2), Christie *et al.* (3), Guetzkow *et al.* (5,6), Leavitt (8), and Shaw (11-15) have shown how differences in communication networks are related to quantity and quality of performance, leadership, organizational developments and different types of satisfactions. Shaw (13), Macy *et al.* (10), and Heise and Miller (7) have shown that the kind of task affects the performances of groups in interaction with the kind of network used. (See Glanzer and Glazer [4] for a more extensive discussion of communication network studies.) It has generally been concluded that, for problems that are routine and repetitive, the more a communication network allows for equal communication opportunities among members, the higher will be the satisfactions, the less will leadership emerge and be recognized, the longer will be the times taken to solve problems, and the greater will be the frequency of errors.

With the exceptions of Luce *et al.* (9) and Shaw (14,15), communication experiments have been of short duration, one hour or less. With the exception of Shaw (14), there has been no attempt to study the developmental behaviors of problem-solving groups for longer time periods. Shaw studied the performance and satisfaction trends of four-person groups working for a short period of time each day for ten days, on fairly complex (arithmetic) problems. In one important sense, Shaw helped answer questions raised about the temporary nature of the effects of working in different communication networks: the effects of prolonged experience on the behaviors of subjects who have had opportunities to rest and perform different activities in between problem-solving attempts. There is, however, another and equally important kind of

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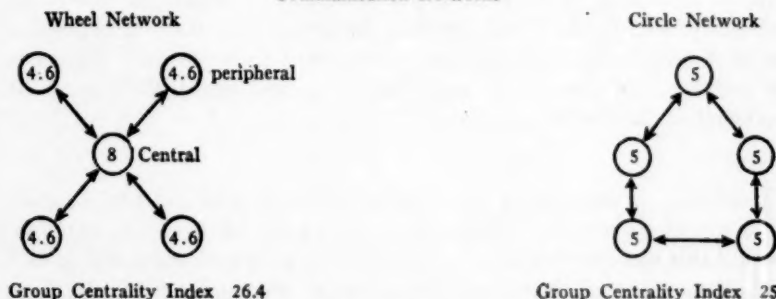
prolonged experience that groups might have. This¹ is the kind in which participants work *continuously* for relatively long periods of time.

PROCEDURE³

Subjects

One hundred male subjects, paid volunteers, drawn from undergraduate classes at Boston University, were randomly assigned to 20 five-man groups.

FIGURE 1
Communication Networks



The Group Centrality Index is a measure of the degree of centralization. It is the sum of the Relative Centrality Indices (8) of the positions. They are the circled numbers in the above diagram. They represent ratios obtained by summing the number of steps to communicate from all positions to all others and dividing the sum by the number of steps to communicate from a given position to all others. The higher the Group Centrality Index, the greater inequality in communication opportunities.

The Wheel has the highest Group Centrality Index of communication networks with two-way channels. It represents the highest degree of inequality in opportunity to communicate. A problem-solving system in which occupants of peripheral positions send information to and receive answers from the occupant of the central position is called a *Central-Hub*. It is the most efficient system possible in the Wheel network for the class of problems used in this study.

The Circle network has the lowest Group Centrality Index. All positions are equal in opportunities to communicate. A problem-solving system in which each member collects information from the all others and decides on the answers himself is called a *Circuit*. The most efficient system possible in the Circle network for the class of problems used in this study is called a *Relay*, which represents the most centralized usage of communication channels. It is a system characterized as follows: a communication channel between two positions is not used; the occupants of these two positions become End-men; each End-man sends information to his one remaining contact, who becomes a Relay-man; each Relay-man sends his and the End-man's information to the occupant of the remaining position who becomes a Center-man; the Center-man decides on an answer, instructs the Relay-men, who in turn, instruct their respective End-men.

³ The procedure was designed to be essentially the same as that used by Leavitt (8) with the exception of a greater number of problems.

Ten groups were used in each of two communication networks, Wheel and Circle (see Figure 1). Subjects were randomly assigned to positions in networks, and were not informed of the kind of network in which they were to work. Before solving problems, subjects read instructions that stressed the importance of working as a team and finding correct solutions as quickly as possible.

Communication Networks

In the Circle network, each member could directly send messages to and receive them from the member to his immediate right and the one to his immediate left. In the Wheel network, the occupant of the central position could directly send messages to and receive them from everyone; occupants of peripheral positions could send messages to and receive them from the occupant of the central position only.

Task

Each subject was given a card, labeled by booth color and trial number, with five of six possible symbols on it. For each trial, there was only one symbol that was common to all five cards. The task of each group was to find the common symbol through the use of written communications only. Subjects could send messages only on their own colored message cards. Each group had to try to solve 60 problems continuously, involving a time period of three to four hours. A trial was ended when all five subjects had registered their answers.

Apparatus

Subjects were seated around a circular table. Each was separated by a vertical partition extending from the center to a foot beyond the edge of the table so that subjects could not see each other. At the center of the table was a five-layer pentagonal box with slots permitting subjects to send messages to and receive them from those with whom they were allowed to communicate. Networks were established by leaving open only certain slots from those connecting everyone. Each booth had six mounted switches, each of which was labeled and represented one of the six possible answers. Switches were separately wired to a master panel on the experimenter's table where depressing the switches was registered. Subjects were allowed to change answers as often as they wished before a trial ended.

Dependent Measures

a. *Times* taken to solve problems were measured in seconds, from the "begin" signal of the experimenter to the point when all subjects had registered answers.

b. *Correct trials* were those in which all five subjects had registered correct answers as their final responses.

c. *Final errors* represented the number of subjects in each network who registered incorrect final answers.

d. *Answer changes* represented the number of changes in responses that subjects made before the end of each trial.

e. *Job, interpersonal and task satisfactions, certainty of answers, and perception of leadership* were measured by questionnaires administered at the end of every fifth trial. Satisfaction and answer certainty items had four alternatives; e.g., very certain, somewhat certain, somewhat uncertain, very uncertain (of the correctness of answers). *Job satisfaction* was indexed by how much a subject said he liked his role in the group problem solving, *interpersonal satisfaction* by how much a subject said he liked other members, and *task satisfaction* by how much a subject said he was interested in the problems he had to solve. *Perception of leadership* refers to the "yes" or "no" judgment of a subject that a leader had existed for the preceding five-trial period. If "yes," subjects were asked to designate the leader.

f. *Messages* were content-analyzed to determine how many communications were used and how groups organized to solve problems.

Hypothesis

Groups in the Wheel network will solve problems more quickly, show lower job, interpersonal and task satisfactions, correctly solve more problems, change answers less frequently, send fewer communications, and have leadership emerge and be recognized more frequently.

RESULTS

Time

Wheel groups took significantly shorter times to solve problems (Table 1, column 1). Groups in both Wheel and Circle networks showed significant decreases over trials. The gradient of time decrease was steeper for the Wheel groups. Tukey's gap test (16) was used to determine when differences between trial means were significant. The results were different from those of shorter-duration studies. Learning, inferred from significant decreases in trial-mean times, continued to appear after many more trials than previous research had taken into account, for groups in both Circle and Wheel networks. In the present findings, significant differences were obtained between trial-mean times up to 30 trials in the Circle and 15 trials in the Wheel networks.

Answer Changes

Wheel groups made significantly fewer changes than Circle groups (Table

1, column 2). Groups in both networks made significantly fewer changes over trial blocks. Wheel groups showed a steeper decrease in changes.

Correct Trials

Wheel groups correctly solved more problems (Table 1, column 3). Neither Wheel nor Circle groups showed increases in correctly solved problems over trial blocks. A likely explanation for the apparent absence of learning in this respect is that, for the kind of task used, the total number of trials that are incorrect (60 minus the number of correct trials) is not large enough to lead to significant differences when they are distributed over such a large number of trials.

Errors

Wheel groups made significantly fewer final errors (Table 1, column 4a). Although Circle groups made significantly more answer changes, the proportion of those that were error corrections was significantly smaller than that of the Wheel group (Table 1, column 4b).

Answer Certainty

Occupants of central positions in Wheel networks were significantly more certain of their answers than were those occupying peripheral positions (Table 2, column 1b) or those in Circle networks (Table 3, column 1b). Occupants of peripheral positions did not differ significantly from Circle members (Table 3, column 1a).

Job Satisfaction

Occupants of central positions in Wheel networks were significantly more satisfied than occupants of peripheral positions (Table 2, column 2b) or Circle members (Table 3, column 2b). Circle members were significantly more satisfied than occupants of peripheral positions in Wheel networks (Table 3, column 2a).

Interpersonal Satisfaction

Occupants of central positions in Wheel networks were significantly more satisfied than occupants of peripheral positions (Table 2, column 3b) and Circle members (Table 3, column 3b). Circle members did not differ significantly from occupants of peripheral positions in Wheel networks (Table 3, column 3a).

Task Satisfaction

Circle members did not differ significantly from occupants of central positions in Wheel networks (Table 3, column 4b). Each showed significantly

TABLE 1
Analyses of Times, Answer Changes, Correct Trials, Final Errors, in Wheel and Circle Communication Networks

[illegible]

X^2	Column 4b	Proportion of Answer Changes that were Error Corrections (+)
	C	W
	+ 113	32
	- 33	1

$X^2 = 6.7$
 $df = 1$
 $p < .01$
 $W > C$

* Time scores were reciprocally transformed and multiplied by 100 to reduce heterogeneity of variance.

•• The analysis was done with blocks of five trials.

*** This was used as the error term for Between Networks.

**** This was used as the error term for Trials and Trials \times Networks. It represents the pooled Group \times Trial \times Network interaction.

TABLE 2

Analyses of Answer Certainty, Job, Interpersonal and Task Satisfactions, and Perception of Leadership in Blocks of Five Trials

Analyses of Variance								
Variables	Column 1a Answer Certainty				Column 2a Job Satisfaction			
	\bar{X} SS.	df	F	p	\bar{X} SS.	df	F	p
C vs. Wc+p *								
Between Networks	2.81	1	3.39	n.s.	4.22	1	2.59	n.s.
Between Groups ** Treated Alike	0.83	18	—	—	1.63	18	—	—
Trial Blocks	0.50	11	6.25	<.01	0.97	11	16.17	<.01
Trial Blocks x Networks	0.10	11	1.25	n.s.	0.06	11	1.00	n.s.
Residual ***	0.08	198	—	—	0.06	198	—	—

Analyses of Variance								
Variables	Column 1b Answer Certainty				Column 2b Job Satisfaction			
	\bar{X} SS.	df	F	p	\bar{X} SS.	df	F	p
Wp vs. Wc *								
Between Networks	5.55	1	6.85	($\begin{smallmatrix} <.05 \\ Wc > Wp \end{smallmatrix}$)	45.72	1	26.58	($\begin{smallmatrix} <.01 \\ Wc > Wp \end{smallmatrix}$)
Between Groups ** Treated Alike	0.81	18	—	—	1.72	18	—	—
Trial Blocks	0.33	11	6.60	<.01	0.21	11	1.75	n.s.
Trial Blocks x Networks	0.11	11	2.20	($\begin{smallmatrix} <.05 \\ Wc > Wp \end{smallmatrix}$)	0.06	11	0.50	n.s.
Residual ***	0.05	198	—	—	0.12	198	—	—

* Symbols c, p, c+p, postscripted to W (Wheel network) indicate data from central positions, peripheral positions, and central and peripheral positions combined, respectively, were used in the analyses. Symbol C represents data from combined positions in the Circle network.

** This was used as the error term for Between Networks.

*** This was used as the error term for Trial Blocks and Trial Blocks x Networks. It represents the pooled Group x Trial Block x Network interaction.

**** The X² data were those trial blocks in which a leader was named and agreed upon by 4/5 members, and are represented by +'. The maximum number of such trial blocks is 120 (12 trial blocks for each of 10 groups in each network).

TABLE 2—(Continued)

Analyses of Answer Certainty, Job, Interpersonal and Task Satisfactions, and Perception of Leadership in Blocks of Five Trials

Analyses of Variance									X ² ****
Variables	Column 3a Interpersonal Satisfaction				Column 4a Task Satisfaction				Column 5 Perception of Leadership
	\bar{X} SS.	df	F	p	\bar{X} SS.	df	F	p	
C vs. Wc+p *									$\frac{Wc}{C+p}$ + $\frac{0 110}{-120 10}$
Between Networks	0.42	1	0.64	n.s.	7.08	1	6.38	($C < .05$ $C > Wc+p$)	
Between Groups ** Treated Alike	0.66	18	—	—	1.11	18	—	—	X ² =199.68 df=1 p<.01 Wc+p>C
Trial Blocks	0.14	11	2.80	<.01	2.80	11	104.00	<.01	
Trial Blocks x Networks	0.06	11	1.20	n.s.	3.08	11	154.00	($C < .01$ $C > Wc+p$)	
Residual ***	0.05	198	—	—	0.02	198	—	—	

Analyses of Variance								
Variables	Column 3b Interpersonal Satisfaction				Column 4b Task Satisfaction			
	\bar{X} SS.	df	F	p	\bar{X} SS.	df	F	p'
Wp vs. Wc *								
Between Networks	19.12	1	42.49	($\begin{smallmatrix} <.01 \\ W_c > W_p \end{smallmatrix}$)	78.49	1	16.70	($\begin{smallmatrix} <.01 \\ W_c > W_p \end{smallmatrix}$)
Between Groups ** Treated Alike	0.45	18	—	—	4.70	18	—	—
Trial Blocks	0.05	11	0.46	n.s.	2.62	11	10.08	<.01
Trial Blocks x Networks	0.06	11	0.55	n.s.	0.20	11	0.77	n.s.
Residual ***	0.11	198	—	—	0.26	198	—	—

* Symbols c, p, c+p, postscripted to W (Wheel network) indicate data from central positions, peripheral positions, and central and peripheral positions combined, respectively, were used in the analyses. Symbol C represents data from combined positions in the Circle network.

** This was used as the error term for Between Networks.

*** This was used as the error term for Trial Blocks and Trial Blocks x Networks. It represents the pooled Group x Trial Block x Network interaction.

**** The X² data were those trial blocks in which a leader was named and agreed upon by 4/5 members, and are represented by +'s. The maximum number of such trial blocks is 120 (12 trial blocks for each of 10 groups in each network).

higher satisfaction than occupants of peripheral positions (Table 3, column 4a; Table 2, column 4b, respectively).

With the exception of task satisfaction (Table 2, column 4a), combining peripheral and central positions in Wheel networks had the effect of obscuring significant differences among occupants of these positions and the members of Circle networks (Table 2, columns 1a, 2a, 3a). All the analyses (excepting those comparing job and interpersonal satisfactions for occupants of central and peripheral positions in Wheel networks, Table 2, columns 3b and 2b, respectively) showed significant trial-block terms. Tukey's gap test revealed that only in task satisfaction did significant differences in means (these represented decreases) extend beyond the sixth trial block. In answer certainty, job and interpersonal satisfactions, there were no such trends. Nearly all the significant differences in trial-block means for these variables occurred among the first three trial blocks, and included both decreases and increases. Members of both networks seemed to have maintained their attitudes formed in the early trials of their problem solving.

Leadership

Members of Wheel groups named and agreed on leaders significantly more than those of Circle groups (Table 2, column 5). In no Circle group was a leader named and agreed on for any trial block (0 in 120 possible opportunities). Wheel groups (with the exception of two, which accounted for six of the ten trial blocks in which leaders were not named and agreed on in the Wheel network) consistently named the occupants of the central positions as leaders, after the first trial block (110 in 120 possible opportunities).

Contents of Messages

Message contents were analyzed into four major categories of units, defined as sentences or meaningful parts of sentences: (a) Organizational activities: inquiries and statements about network structure, planning activities, specific and general work proposals; Wheel groups devoted 3.86 per cent of their messages, and Circle groups, 0.70 per cent, to these activities. (b) Operating task activities: messages concerning pre-coding, asking for and giving information about problems, asking for and giving answers to problems; Wheel groups devoted 90.76 per cent to these activities, of which 87.28 per cent was spent in giving information and answers. Circle groups devoted 93.67 per cent to these activities, of which 88.31 per cent was spent in giving information and answers. (c) Evaluations: about work procedures and other activities of members; Wheel groups devoted 0.92 per cent, and Circle groups, 0.65 per cent to these activities. (d) Waste-basket: messages concerning

TABLE 3
Analyses of Answer Certainty, Job, Interpersonal and Task Satisfaction, in Blocks of Five Trials

Variables	Analyses of Variance																			
	Column 1a Answer Certainty				Column 2a Job Satisfaction				Column 3a Interpersonal Satisfaction				Column 4a Task Satisfaction							
Terms	\bar{X}	SS.	df	F	p	\bar{X}	SS.	df	F	p	\bar{X}	SS.	df	F	p	\bar{X}	SS.	df	F	p
C vs. Wp*																				
Between Networks	1.48	1	1.44	****		11.33	1	5.72	($<.05$)	($C > Wp$)	2.33	1	2.28	****		18.51	1	25.27	($<.01$)	($C > Wp$)
Treated Groups **	1.03	18	—	—	—	1.98	18	—	—	—	1.02	18	—	—	—	0.72	18	—	—	—
Trial Blocks	0.44	11	4.40	<.01		0.61	11	6.10	<.01		0.13	11	2.60	<.01		4.34	11	36.17	<.01	
Trial Blocks x Networks	0.11	11	1.10	****		0.11	11	1.10	****		0.08	11	1.60	****		0.19	11	1.58	****	
Residual ***	0.10	198	—	—	—	0.10	198	—	—	—	0.05	198	—	—	—	0.12	198	—	—	—
Variables	Column 1b Answer Certainty				Column 2b Job Satisfaction				Column 3b Interpersonal Satisfaction				Column 4b Task Satisfaction							
Terms	\bar{X}	SS.	df	F	p	\bar{X}	SS.	df	F	p	\bar{X}	SS.	df	F	p	\bar{X}	SS.	df	F	p
C vs. Wc*																				
Between Networks	12.79	1	16.83	($<.01$)	($Wc > C$)	11.53	1	26.21	($<.01$)	($Wc > C$)	7.74	1	28.67	($<.01$)	($Wc > C$)	20.77	1	3.73	****	
Treated Groups **	0.76	18	—	—	—	0.44	18	—	—	—	0.27	18	—	—	—	5.57	18	—	—	—
Trial Blocks	0.16	11	2.67	<.01		0.62	11	10.33	<.01		0.16	11	2.00	<.05		3.19	11	16.79	<.01	
Trial Blocks x Networks	0.12	11	2.00	($<.05$)	($Wc > C$)	0.06	11	1.00	****		0.12	11	1.50	****		0.28	11	1.47	****	
Residual ***	0.06	198	—	—	—	0.06	198	—	—	—	0.08	198	—	—	—	0.19	198	—	—	—

* Symbols c, p, c+p, postscripted to W (Wheel network) indicate that data from central positions, peripheral positions, and central and peripheral positions combined, respectively, were used in the analyses. Symbol C represents data from combined positions in the Circle network.

** This was used as the error term for Between Networks.

*** This was used as the error term for Trial Blocks and Trial Blocks x Networks. It represents the pooled Group x Trial Block x Network interaction.

**** This indicates that the result was not significant.

irrelevant matters; Wheel groups devoted 4.63 per cent, and Circle groups, 4.92 per cent, to these activities.

Number of Messages

In addition to times taken to solve problems, the number of messages that a group uses is an indicator of the amount of effort it expends. The number of message units can be used to represent how "hard" a group had to work. Analyses of variance (on blocks of five trials) revealed that Circle groups sent significantly more messages. Both Wheel and Circle groups sent significantly fewer messages over trial blocks. Tukey's gap test revealed that for Wheel groups there were no significant differences among trial blocks after the fourth. The differences were especially small after the sixth. It was after this block that the number of message units per problem was close to or equal to eight, the minimum number needed for a solution. The gap test on number of message units sent by Circle groups revealed that, after the sixth trial block, there were no significant differences among blocks. Circle groups always used many more messages than the minimum.

Both Wheel and Circle groups showed increased efficiency, in that fewer message units were needed to solve problems without sacrificing quality (no changes in number of correctly solved problems; see Table 1, column 3). Wheel groups reached maximum efficiency (used the minimum number of messages). Circles did not.

Kind and Stability of Organization

The procedure for determining kind and stability of organization was similar to that used by Guetzkow and Simon (6). Every trial for each group was analyzed separately for the presence of information and answer units.⁴ Blocks of five trials were analyzed to determine which channels were always used (used at least once on each trial of a block), intermittently used (used at least on one, but not on all five trials of a block), and never used. For each group, there was obtained a series of twelve blocks, representing all 60 trials, the inspection of which enabled the investigators to locate role differentiation where it developed, on the basis of how positions were consistently and strategically placed in the information and answer exchange patterns.

a. *Information exchange patterns in Circle groups.* The information exchange patterns of the Circle groups began and remained as the kind in which each subject sent information to and received it from everyone with whom he was in contact. Through continuous exchanges of this kind, each subject col-

⁴ A typical information unit might be: "Here are my symbols: ———." A typical answer unit might be: "Symbol ——— is the correct answer."

lected the information of all other group members. There was no role differentiation. There were centers neither for receiving nor for sending information. With very few exceptions, Circles always used all channels for exchanging information.

b. *Answer exchange patterns in Circle groups.* The answer exchange patterns were very similar to those of information exchange. Almost all channels were used for the first six trial blocks. The major difference between the information and answer exchange patterns was that, although in both patterns nearly all channels were used, they were used intermittently in the answer exchanges, and always in the information exchanges.

c. *Information and answer exchange patterns in Wheel groups.* By the second trial block in 50 per cent, and by the fifth block in 100 per cent of the groups, occupants of central positions had stopped sending information and only sent answers. Occupants of peripheral positions, at comparable points and in similar proportions, respectively, stopped sending answers and only sent information. From the fifth trial block on, for any given Wheel group, the given channel was either used consistently or not at all.

Circle groups developed Circuit problem-solving systems. Wheel groups developed Central-Hub systems (see Figure 1).

DISCUSSION

The results of studies of shorter duration were generally substantiated: Groups in the Wheel network solved problems more quickly, correctly solved more problems, had more leadership emerge and be recognized, changed answers less frequently, and sent fewer messages. Certain results are different, however, and need to be examined.

Theoretically, Wheel and Circle groups of five members can be equally productive. In each network, if they were to perform optimally, groups could solve problems using only eight message units (1). Assuming similar message-sending rates, theoretically the times taken to solve problems would not be expected to be significantly different between Wheel and Circle groups. In actuality, Wheel groups reached optimum performance. Circle groups did not.

Learning

The results revealed that: (a) learning continued to take place in both communication networks for a considerably longer period than had been expected on the basis of studies of shorter duration; (b) two kinds of learning occurred, that represented by the development of a problem-solving system, and that represented by progressively more efficient operations within a problem-solving system.

Differences in Learning Rates

The rates of learning in the Wheel groups were faster than those of Circle groups with respect to times taken to solve problems, number of message units, and answer changes. Several factors probably accounted for these differences. As was reported, Wheel groups developed the *central-hub* system, the best possible system, for the tasks facing them. Circle groups did not. The most efficient system possible within the Circle network would have been the Relay (see Figure 1). In addition, Wheel groups stabilized their systems earlier; that is, they ceased using unnecessary channels earlier, particularly for answer exchanges. It is reasonable to assume, other things being equal, that once members of a group have adopted and stably used a problem-solving system, they can concentrate their efforts in the direction of increasing their operating efficiency. Thus, if groups in Wheel networks develop their problem-solving systems earlier, they will reach peak efficiency sooner.

The development of clearly recognized and agreed-upon leadership in the central-hub systems in Wheel networks also contributed to the differences in rates of learning. The more a leader is clearly recognized and agreed upon (this was characteristic of no Circle group), the more likely will other members accept influence attempts by him: procedures, answers, etc. Less energy and time will be spent by other members in duplicating the functions of the leader: figuring out answers for themselves, checking on others (once the leader has approved information by passing it on), and trying to set up variations in problem-solving procedures according to their own idiosyncratic evaluations.

The transfer of certain role responsibilities to leaders, in problem-solving situations of this kind, leads to a condition in which non-leader members can devote their time and efforts to doing their particular jobs better and more quickly. A given amount of effort, when devoted to fewer, non-redundant activities, would be expected to lead more quickly to greater efficiency.

Satisfactions and Answer Certainty

The results suggest that, when there is role differentiation, analyses of the responses of those performing different roles is the only appropriate means of characterizing satisfaction and certainty of answer. Occupants of central positions in Wheel networks were significantly different from those occupying peripheral positions on all the satisfaction and answer certainty variables. Combining the data from two such positions whose occupants differed significantly in their responses would result in scores that would not accurately represent either position (as it did in this study). Comparisons with other networks, consequently, would yield unrepresentative results.

In this study, roles and positions were confounded. Occupancy of a par-

ticular position was always followed by performance of a particular role. Those who occupied central positions in Wheel networks became leaders. Those who occupied peripheral positions became followers. Members in Circle groups began and remained as independent problem-solvers. Because of this, the question of whether satisfaction is related to position in network, or role, or an interaction of the two, cannot be answered from the results of this study.

Limitations of Data on Short-Term Performances

Studying groups for short-time periods can lead to premature estimates about performance stabilization and, consequently, to spurious inferences about learning. Learning of a more gross nature might be detected because of an exaggerated variance based on scores representing the early and most variable periods of work. Learning might be considered, prematurely, to have ceased. By increasing the number of problems, enough data can be obtained to prevent such occurrences.

SUMMARY

This study was concerned with the effects of continued practice on the behaviors of groups trying to solve a relatively large number of problems. Ten groups of five men were assigned to each of two communication networks: (a) a Wheel, and (b) a Circle. Each group tried to solve 60 problems consecutively. Questionnaires were administered after each block of five trials. These measured: answer certainty, satisfaction with job, other members and task, and perception of leadership. The procedure was intended to approximate as much as possible the conditions of the Leavitt study (8), with the exception of using many more trials.

Differences between groups in Wheel and Circle networks were found: Wheel groups took shorter times to solve problems, had more correctly answered problems, made fewer answer changes, made fewer final errors, sent fewer messages, and had more answer changes that were error corrections. Groups in both networks learned to solve problems with fewer messages, fewer answer changes, and in shorter times. Wheel groups, however, learned more quickly.

Leadership was not recognized or agreed upon at all in the Circle groups. It was almost completely recognized and agreed upon in the Wheel groups.

Differences between Wheel and Circle groups were not significant for the variables of satisfaction with job, other members and the task. Wheel groups were more certain of their answers. When analyses were done according to positions in networks, it was found that occupants of central positions in Wheel networks were significantly more satisfied than Circle members, who

were more satisfied with their jobs, fellow members and tasks than occupants of peripheral positions. Occupants of central positions were more certain of their answers than occupants of peripheral positions. Thus, combining data from different positions led to results that obscured the effects of the networks.

Wheel groups developed the Central-Hub problem-solving system, the most efficient one possible in the Wheel network for the class of problems facing them. Circle groups developed Circuit problem-solving systems. The most efficient one possible within the limits of the Circle network would have been the Relay. Wheel groups developed their systems earlier and increased their operating efficiency at a faster rate. Groups in both networks continued to learn over longer periods than had been found in studies of shorter duration. Studies of shorter duration were considered to have the potential weakness of leading to premature estimates performance stabilization and, consequently, spurious inferences about learning.

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Toward a Methodological Codification: The Shotgun and the Saltshaker

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In an area like social psychology where the idea of theory is highly valued, it is not surprising to find that approaches that do not have the elegance ordinarily expected of "hard theory" are often both disliked and maligned. One of the frequent whipping boys is identified as the "shotgun approach." This paper attempts to outline briefly the basis for use of the shotgun approach.

Consider, as a convenient example, a recent criticism of the shotgun approach in a review of several social psychology monographs. Having indicated that the authors had discussed correlations that had been found, the reviewer proceeded with the following statement (1):

And this, in turn, is the frightening part of such a 'shotgun approach.' Conservatively estimated, the number of correlation coefficients presented in this monograph is 3,963. By chance, one would expect 40 to be significant at the 1%-level and another 160 to be significant at the 5%-level. Conclusions based on such 'statisticizing' must be tenuous at best, even if founded on sound hypotheses. When much of the theorizing is after the fact (as in the present monograph), one wonders whether or not it really has been worth the effort.

Here two questions raised frequently in regard to the shotgun approach are made explicit. The first is that, given the multitude of findings that occur when a great many questions are asked, how does one find out which ones are the stable or important ones? And, second, is it worth theorizing after the fact?

Let us attempt to answer the first question by giving attention to a major alternative. Suppose an investigator is concerned with a given set of facts about which he wishes to know more, say, in terms of concomitants, precursors, and consequences (presuming this includes everything). We have to acknowledge that (a) he must have some information about the facts he wishes to investigate or he wouldn't be able even to raise the issue. Then, being a reasonable scientist, (b) he will go to the literature or to authorities that are otherwise available, and he will find out what the status of knowledge is in regard to the facts. Having looked over the area, he selects what appears to be the "best" theory. Conceivably there may be only one theory covering the facts, but more likely there will be several. The permissiveness of science allows him to take certain liberties and, on the basis of *his* judgment, to formulate his revision of *the* theory.

Obviously, the theory will not be "hard theory." Besides being limited in range, the assumed variables are, of course, inferentially related to the *ad hoc*

variables of the researcher, and the goodness of the inference is crucial to hypothesis testing. Having the theory, one he has either accepted or redefined, he may observe phenomena to test the theory; that is, if certain conditions can be specified at a point in time, certain consequences *must* follow by the hypothesis derived from the theory.

Now either of several things can occur. First, the findings may be in accord with the hypothesis and satisfy a given statistical significance for a hypothesis test. Second, the findings may not satisfy the arbitrary test. Third, not only may the second condition obtain, but the findings may appear to contradict the hypothesis. Suppose the results are in accord with his hypothesis. The researcher is then faced with several additional alternatives. For example, he may ask how well he can predict the phenomena that concern him (the consequences) and, if he has not predicted them at the maximum level that would be desired, he must then account for this insufficient prediction. This may mean improvement of measures, or it may mean revision of the theory to take additional factors into account. He is, however, in business. But to improve the prediction of the consequences he must seek further leads. At this point he can go back to his original work of reviewing the literature, speaking with authorities, and in other ways examining what the relevant variables are for his research, and on the basis of these, revise his theory and go over the entire cycle again, *ad infinitum*. We must emphasize here that this iterative procedure must always refer to the same bases of knowledge that the first trial involved, plus the new experience, although certainly additional things may be added and others dropped in the subsequent procedures. The researcher must also examine the question of what alternate theories could have predicted the results, and exploring this question should lead to further development of hypotheses.

If the research had negative findings, presumably this could occur from errors of measurement, in which case he may wish to revise his procedure. He may have had some faulty logic involved in his hypothesization, or his theory might have been inappropriate. In any event, he has the same problem of going back and revising, just as though he had had findings, except that in this case he is not able purposively to improve the prediction of the consequences, because he has not been able to predict at all.

The last alternative is the one of findings in the direction opposed from those that were hypothesized. This, of course, is the most charming type of result, since it permits a paper in which one blasts away, indicating how foolish others may have been to have had any kinds of findings contrary to this and how gullible they were to have reported them. The virtuosity permitted under findings contrary to those hypothesized is not paralleled in any other way in science. In any event, the researcher must also undertake the

iterative procedure, but from this peculiar point, that the empirical facts contradict the "best" theory.

Let us see how *the notion of the shotgun* now relates to the research process. In the elegance of his theory, the investigator has asked only those questions that are necessary to test the theory. If he has findings in accord with the theory, we note that to improve the prediction of the consequences (hypothesized by the theory) or to differentiate his theory from others that would predict similar findings, he must inevitably revert to other measures and possibly go outside his initial scope of inquiry. This inevitable operation also is involved if he gets negative findings. Now the question forcefully arises, "Why did he restrict his initial question asking to what obviously was a prejudgment about the very situation he wished to investigate?"

The shotgun researcher is just as competent as his fellow in reading the literature and in appealing to authorities for leads, but, instead of assuming or formulating a "best" theory as such, he notes the things that have been indicated to be relevant to the class of phenomena he wishes to observe. Among these, let us assume, are those factors that have been built into his colleague's theory. The shotgun researcher, though, does not have the confidence of his colleague, so he says that possibly this rather elegant theory, while brilliant, may still be factually inappropriate. Thus, *without necessarily foregoing preferences*, he observes that there are theories in the speculative sense. His concern becomes one of asking questions relevant within the scope of the problem. In fact, he is not able to define the problem too well; he may even introduce certain alternative definitions of what the problem might be. Therefore, he observes the consequences in not one, but in several ways.

After having done his research, the shotgun researcher has reached the point where he has a mass of data corresponding to the inclusive set of research questions. It can't give answers in accord with the hypothesis or contrary to the hypothesis. At best, what can be said is: These classes of phenomena that might have been called consequences had we had a theory appear equivalent and these do not; they appear to be ordered in these ways; then for each of these consequences the predictability is such and such on the basis of these variables. The shotgun researcher, thus, is left with the job of unscrambling his findings. This may not be an elegant procedure, but, on the other hand, why should it be?

At this point the shotgun researcher has a body of empirical generalizations about which he can speculate, just as his colleague might speculate about how to improve his observations after he has found data in accord with his hypothesis. The advantage he has is that he may go to his data immediately to reject much that his colleague must now continue to test in his iterative procedure. Note, however, that the shotgun researcher must, just like any other scientist, ask: "Whither do I go?" He has, however, covered many of

the avenues that will plague his colleague for a long time. On the other hand, he must (as must also his colleague) test the stability and generality of his findings through replication.

The second point made earlier was whether or not it is worth while building theory on a *post hoc* basis. The question that really arises is whether there is any alternative to this. In the shotgun method, if all relevant questions that could be included have indeed been included, the findings should represent a substantial and relatively systematic panel of empirical observations. What could be a better basis on which to formulate generalizations and to speculate about additional factors that might be involved?

There is one final point that needs to be clarified. Apparently the most devastating thing that can be said about a researcher who uses something that can be labeled the shotgun method is that occasionally he throws in "everything including the kitchen sink." As in the case of a lot of other straw men, finding the culprit in the literature is more difficult than recalling incidents of hearing this criticism. What is important, however, is that in science, if one cannot predict the criterion (that is, if there is no known variance that can be controlled in regard to an observed class of phenomena), or if the limits of the predictable variance have been found and improvement is not available in the known sources of variance, then by definition the known sources of variance are taken into account. There no longer exists a rationale for testing additional variance except for excluding rejected solutions. By definition, all tests of additional variance are at random within the scope of possible solutions of unknown or untested value until additional variance is encountered, and then, of course, one may again test in the neighborhood of the new source of variance to see if the criterion can be improved. What needs to be emphasized is that, when one is looking for new sources of variance, this is explicitly throwing in the kitchen sink, the garbage pail, or whatever else is available that has not been looked at yet. It seems trivial to emphasize, and yet it is very important: science permits within its scope the prediction of phenomena without necessarily understanding them (say, on a reductionistic base). On the other hand, by definition, it is not possible to understand in a scientific sense without being able to predict.

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The first of these is the fact that the world is not a uniform whole, but a collection of many different parts, each of which has its own characteristics and its own history. This is the case with the human world, which is made up of many different peoples, each with its own customs, beliefs, and ways of life. It is also the case with the natural world, which is made up of many different plants and animals, each with its own characteristics and its own history. This diversity is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The second of these is the fact that the world is not a static whole, but a dynamic whole, which is constantly changing and developing. This is the case with the human world, which is constantly changing and developing as a result of the actions of the people who live in it. It is also the case with the natural world, which is constantly changing and developing as a result of the actions of the forces of nature. This dynamism is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The third of these is the fact that the world is not a separate whole, but a part of a larger whole, which is the universe. This is the case with the human world, which is a part of the larger whole of the universe. It is also the case with the natural world, which is a part of the larger whole of the universe. This fact is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The fourth of these is the fact that the world is not a perfect whole, but an imperfect whole, which is full of many different kinds of imperfections. This is the case with the human world, which is full of many different kinds of imperfections, such as poverty, disease, and war. It is also the case with the natural world, which is full of many different kinds of imperfections, such as earthquakes, hurricanes, and droughts. This imperfection is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The fifth of these is the fact that the world is not a simple whole, but a complex whole, which is made up of many different parts, each of which is itself a complex whole. This is the case with the human world, which is made up of many different peoples, each of which is itself a complex whole. It is also the case with the natural world, which is made up of many different plants and animals, each of which is itself a complex whole. This complexity is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The sixth of these is the fact that the world is not a random whole, but a whole that is governed by certain laws. This is the case with the human world, which is governed by certain laws, such as the laws of morality and the laws of justice. It is also the case with the natural world, which is governed by certain laws, such as the laws of physics and the laws of chemistry. This fact is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The seventh of these is the fact that the world is not a whole that is separate from the human world, but a whole that is part of the human world. This is the case with the natural world, which is part of the human world. It is also the case with the universe, which is part of the human world. This fact is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The eighth of these is the fact that the world is not a whole that is separate from the human world, but a whole that is part of the human world. This is the case with the natural world, which is part of the human world. It is also the case with the universe, which is part of the human world. This fact is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The ninth of these is the fact that the world is not a whole that is separate from the human world, but a whole that is part of the human world. This is the case with the natural world, which is part of the human world. It is also the case with the universe, which is part of the human world. This fact is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

The tenth of these is the fact that the world is not a whole that is separate from the human world, but a whole that is part of the human world. This is the case with the natural world, which is part of the human world. It is also the case with the universe, which is part of the human world. This fact is one of the most important features of the world, and it is one of the reasons why the world is so interesting and so beautiful.

